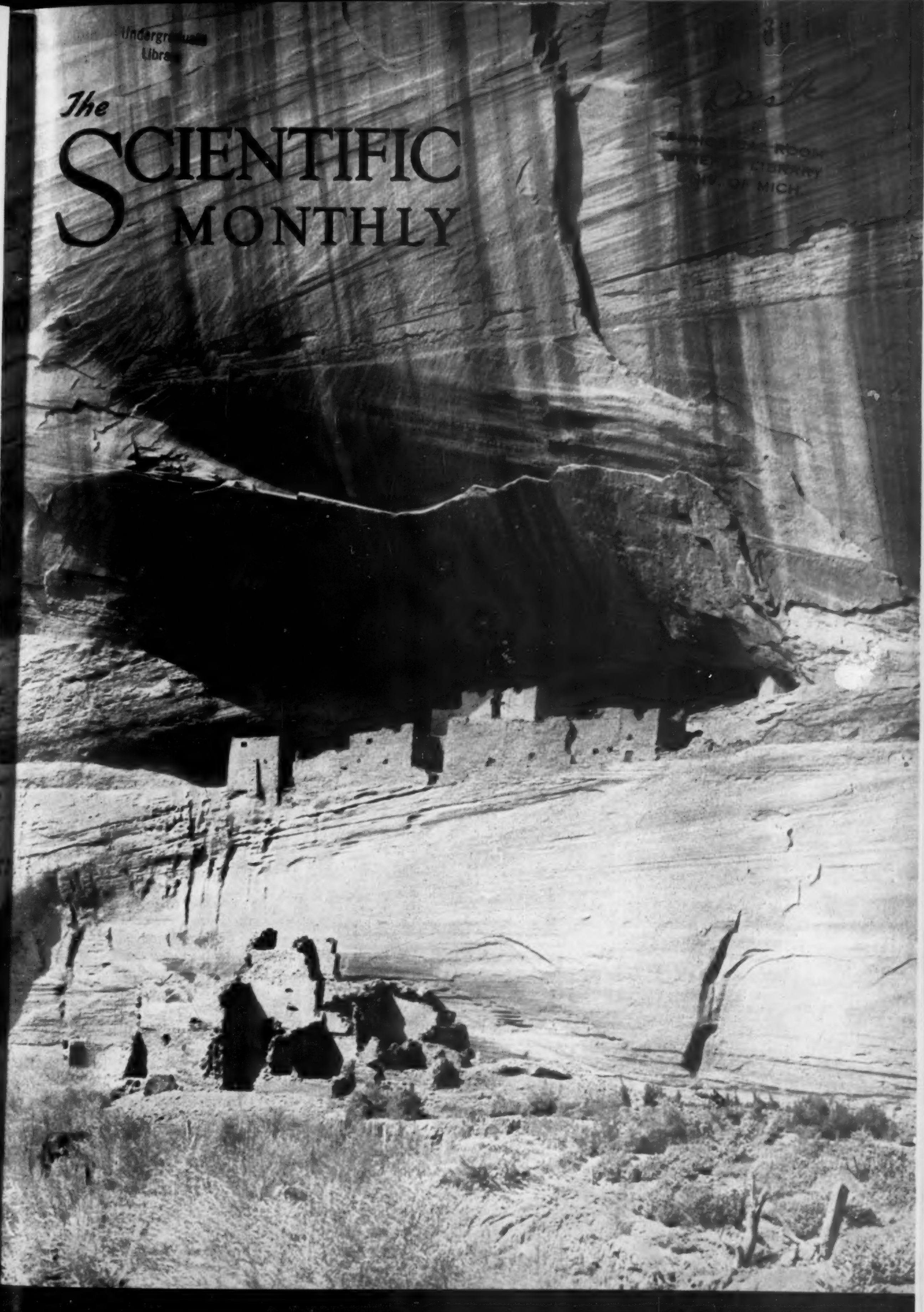
The SCIENTIFIC MONTHLY



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JANUARY 1950


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THE SCIENTIFIC MONTHLY

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Contents

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Meetings

Jan. 3–6. American Meteorological Society (30th Anniversary). Hotel Jefferson, St. Louis, Mo.
Jan. 10–12. AEC-New York University Conference on Industrial and Safety Problems of Nuclear Technology. New York City.
Jan. 18–20. American Society of Civil Engineers (Annual). New York City.
Jan. 23–27. American Society of Heating and Ventilating Engineers (Southwest Air-Conditioning Exposition). Dallas.
Jan. 30–Feb. 3. American Institute of Electrical Engineers (Winter). New York City.
Feb. 15–17. Pittsburgh Conference on Analytical Chemistry and Applied Spectroscopy. William Penn Hotel.
Feb. 26–Mar. 1. American Institute of Chemical Engineers. Rice Hotel, Houston.
Feb. 27–Mar. 2. American Society for Testing Materials. William Penn Hotel, Pittsburgh.
Mar. 8–10. American Petroleum Institute (Southwestern). Adolphus Hotel, Dallas.
Mar. 27–29. Western Petroleum Refiners Association (Annual). Plaza Hotel, San Antonio.
Mar. 28–31. National Plastics Exposition. Navy Pier, Chicago.
Mar. 29–31. American Petroleum Institute (Mid-Continent). Skirvin Hotel, Oklahoma City.

Science and Technology

(From the Month's News Releases)

New Publications

Volume 1 of the National Bureau of Standards Circul 467, entitled *Atomic Energy Levels,* is a critically eval ated compilation of all known data on the energy leve of elements of atomic number 1 through 23. Designed meet the needs of workers in nuclear and atomic physic astrophysics, chemistry, and industry, it is an up-to-da compendium of all energy levels for these elements, excl sive of those due to the hyperfine structure ascribed atomic nuclei. 352 large 2-column pages. Available fro the Superintendent of Documents, Washington, D. C $2.75.

Proceedings of the Technical Session on Bone Char, report of the meeting held under the joint sponsorship the National Bureau of Standards and industries suppor ing bone-char and other solid-absorbents research, con tains the fourteen formal papers presented at the Sessio and subsequent discussions. 321 large-size pages, illus trated with figures, charts, and half tones. Available fro J. M. Brown, Bone Char Research Project, Inc., c/ Revere Sugar Refinery, Charlestown 29, Massachusett $2.00.

Nutritional Data, compiled by Harold A. Wooster, Jr and Fred C. Blanck, of the Mellon Institute, contai comprehensive information on foods, with emphasis o vitamins, essential elements, intermediary metabolism, an many other topics. Published and distributed gratis b H. J. Heinz Co., P. O. Box 57, Pittsburgh, Pennsylvani

The Old Farmer's Almanac, 1950. Robert B. Thoma 120 pp. Illus. 25¢ from Yankee, Inc., Dublin, N. H

Alcohol

According to a report recently made to the America Chemical Society, a new method of extracting alcoh from the South American cassava plant has been devise In this process, starch accumulated in the roots of th cassava can be converted to fermentable sugars by sub merged-culture fungal enzymes. Cassava alcohol is a high grade product and is used in the finest beverages an perfumes.

Better Reading

The Reading Accelerator, designed not only for th problem reader and remedial reading work, but for an one who wants to read well, will train those who do no have vocabulary or psychological difficulties to read fro 100 to 500 per cent faster, yet comprehend more full Average and excellent readers can improve their abilitie in greater ratio than the poor reader. Work already don indicates that training on the Accelerator results in higher level of social adjustment and greater psychologica motivation than the reader previously possessed. Th machine is designed to be used with any type of readin material and does not require preparation or mutilatio of pages. It has a wide range of word-per-minute speed and is accompanied by a calculator that allows the ma chine to be set for any word-per-minute rate on any typ of material. Skill books, reading manuals, reading test and free consultant service on reading-training program are available as part of a complete reading program Training personnel or additional staff members are no necessary for any institution that wishes to install a read ing-training program.

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Atomic Hydrogen as an Aid to Industrial Research

IRVING LANGMUIR

First industrial chemist in this country to receive the Nobel Prize, Dr. Langmuir (Ph.D., Göttingen, 1906) has been associated with General Electric research since 1909. His distinguished career as a scientist is too well known to need recounting here. The present article is Chapter VII of his new book, Phenomena, Atoms and Molecules, *published by Philosophical Library on January 12, and is based on an address by the author upon being awarded the Perkin Medal.*

I BELIEVE the primary object of the Perkin Medal is to do honor to the memory of Sir William Perkin, that pioneer who devoted himself to pure scientific research after having led in the industrial applications of research for fifteen years. This object is best attained by encouraging the kind of research that he valued so highly. The medal should thus be regarded, not as a reward for accomplishment nor as a prize to stimulate competition in research, but rather as a means of directing attention to the value of research and to the methods of research that are most productive. Having this in mind, I am going to tell you, although somewhat reluctantly, the history of some of my own work, in so far is it illustrates a method of industrial research that has proved valuable.

Two Types of Industrial Research

The leaders of industries are frequently conscious of the need of improvement in their processes, and even of the need of new discoveries or inventions which will extend their activities. It is thus logical, and often extremely profitable, to organize research laboratories to solve specific problems. Efficiency requires that the director shall assign to each worker a carefully planned program. Experiments which do not logically fit in with this program are to be discouraged. This type of industrial research, which should often be called engineering rather than research, has frequently been very successful in solving specific problems, but usually along lines already foreseen.

This method, however, has serious limitations. Directors are rare who can foresee the solutions sufficiently well to plan out a good campaign of attack in advance. Then, too, the best type of research man does not like to be told too definitely what must be the objects of his experiments. To him scientific curiosity is usually a greater incentive than the hope of commercially useful results. Fortunately, however, with proper encouragement, this curiosity itself is a guide that may lead to fundamental discoveries, and thus may solve the specific problems in still better ways than could have been reached by a direct attack; or may lead to valuable by-products in the form of new lines of activity for the industrial organization.

Of course, no industrial laboratory should neglect the possibilities of the first and older method of organized industrial research. I wish, however, to dwell upon the merits of the second method, in which pure science or scientific curiosity is the guide.

History of the Gas-filled Lamp

I first entered the Research Laboratory of the General Electric Company in the summer of 1909, expecting in the fall to return to Stevens Institute, where I had been teaching chemistry. Instead of assigning me to any definite work, Dr. Whitney suggested that I spend several days in the various rooms of the laboratory, becoming familiar with the work that was being done by the different men. He asked me to let him know what I found of most interest as a problem for the summer vacation.

A large part of the laboratory staff was busily engaged in the development of drawn tungsten wire made by the then new Coolidge process. A serious difficulty was being experienced in overcoming the "offsetting" of the filaments, a kind of brittleness which appeared only when the lamps were run on alternating current. Out of a large number of samples of wire, three had accidentally been produced which gave lamps that ran as well with alternating as with direct current, but it was not known just what had made these wires so good. It seemed to me that there was one factor that had not been considered—that is, that the offsetting might possibly be due to impurities in the wire in the form of gases. I therefore suggested to Dr. Whitney that I would like to heat various samples of wire in high vacuum and measure the quantities of gas obtained in each case.

In looking through the laboratory I had been particularly impressed with the remarkably good methods that were used for exhausting lamps. These methods were, I thought, far better than those known to scientific research workers. My desire to become more familiar with these methods was undoubtedly one of the factors that led me to select for my first research an investigation of the gas content of wires.

After starting the measurements that I had planned, I found that the filaments gave off surprisingly large quantities of gas. Within a couple of weeks I realized that something was entirely wrong with my apparatus, because from a small filament in a couple of days I obtained a quantity of gas which had, at atmospheric pressure, a volume 7,000 times that of the filament from which it appeared to have come; and even then there was no indication that this gas evolution was going to stop. It is true that in the literature—for example, in J. J. Thomson's book on the *Conduction of Electricity through Gases*—one found many statements that metals in vacuum give off gases almost indefinitely, and that it is impossible to free metals from gas by heating. Still I thought that 7,000 times its own volume of gas was an entirely unreasonable amount to obtain from a filament. I spent most of the summer in trying to find where this gas came from, and never did investigate the different samples of wire to see how much gas they contained. How much more logical it would have been if I had dropped the work as soon as I found that I would not be able to get useful information on the "offsetting" problem by the method that I had employed.

What I really learned during that summer was that glass surfaces which have not been heated a long time in vacuum slowly give off water vapor, and this reacts with a tungsten filament to produce hydrogen, and also that the vapors of vaseline from a ground-glass joint in the vacuum system give off hydrocarbon vapors, which produce hydrogen and carbon monoxide.

That summer's work was so interesting that I dreaded to return to the comparative monotony of teaching and gladly accepted Dr. Whitney's offer to continue at work in the laboratory. No definite program of work was laid down. I was given first one assistant and then others to continue experiments in the sources of gas within vacuum apparatus, and a study of the effects produced by the introduction of various gases into tungsten filament lamps. The truth is that I was merely curious about the mysterious phenomena that occur in these lamps. Dr. Whitney had previously found that gases have a habit of disappearing in lamps, and no one knew where they went to, so I wanted to introduce each different kind of gas that I could lay my hands on into a lamp with a tungsten filament and find out definitely what happened to that gas.

It was the universal opinion among the lamp engineers with whom I came in contact that if only a much better vacuum could be produced in a lamp a better lamp would result. Dr. Whitney, particularly, believed that every effort should be made to improve the vacuum, for all laboratory experience seemed to indicate that this was the hopeful line of attack on the problem of a better lamp. However, I really didn't know how to produce a better vacuum, and instead proposed to study the bad effects of gases by putting gases in the lamp. I hoped that in this way I would become so familiar with these effects of gas that I could extrapolate to zero gas pressure, and thus predict, without really trying it, how good the lamp would be if we could produce a perfect vacuum.

This principle of research I have found extremely useful on many occasions. When it is suspected that some useful result is to be obtained by

avoiding certain undesired factors, but it is found that these factors are very difficult to avoid, then it is a good plan to increase deliberately each of these factors in turn so as to exaggerate their bad effects, and thus become so familiar with them that one can determine whether it is really worth while avoiding them. For example, if you have in lamps a vacuum as good as you know how to produce, but suspect that the lamps would be better if you had a vacuum, say, 100 times as good, it may be the best policy, instead of attempting to devise methods of improving this vacuum, to spoil the vacuum deliberately in known ways, and you may then find that no improvement in vacuum is needed, or just how much better the vacuum needs to be.

During these first few years, while I was thus having such a good time satisfying my curiosity and publishing scientific papers on chemical reactions at low pressures, I frequently wondered whether it was fair that I should spend my whole time in an industrial organization on such purely scientific work, for I confess I didn't see what applications could be made of it, nor did I even have any applications in mind. Several times I talked the matter over with Dr. Whitney, saying that I could not tell where this work was going to lead us. He replied that it was not necessary, as far as he was concerned, that it should lead anywhere. He would like to see me continue working along any fundamental lines that would give us more information in regard to the phenomena taking place in incandescent lamps, and that I should feel myself perfectly free to go ahead on any such lines that seemed of interest to me. For nearly three years I worked in this way with several assistants before any real application was made of any of my work. In adopting this broad-minded attitude Dr. Whitney showed himself to be a real pioneer in the new type of modern industrial research.

For my study of the effect of gases, I had to devise new types of vacuum apparatus. I needed particularly to be able to analyze the small quantities of gas in the tungsten lamp. With some of this special apparatus I was able to make a practically complete quantitative analysis of an amount of gas which would occupy about 1 cu mm at atmospheric pressure. In this sample we could determine the percentages of oxygen, hydrogen, nitrogen, carbon dioxide, carbon monoxide, and the inert gases.

In regard to the fate of the different gases which I introduced into the lamp bulb, I found that no two gases acted alike. Oxygen attacked the filament and formed tungstic oxide, WO_3. That seemed simple enough, but the kinetics of the reaction presented many features of considerable scientific interest.

In studying the effect of hydrogen very peculiar phenomena were observed. A limited amount of hydrogen disappeared and became adsorbed on the bulb, where it remained in a chemically active form, which was capable of reacting with oxygen at room temperature even long after the tungsten filament had been allowed to cool. This suggested hydrogen atoms and seemed to confirm some conclusions that I had already drawn from observations on the heat losses from tungsten filaments in hydrogen at atmospheric pressure. In making squirted tungsten filaments, and sometimes in cleaning the drawn wire, filaments were heated in this manner in hydrogen. Because tungsten filaments melt at a temperature more than 1,500° higher than platinum, it had seemed to me that tungsten furnished a tool of particular value for the scientific study of phenomena in gases at high temperatures. From my work on lamps I knew approximately the relation between the resistance of tungsten wire and its temperature, and could thus use a tungsten wire as a kind of resistance thermometer. By connecting a voltmeter and an ammeter to the tungsten filament which was being heated in hydrogen, I could determine the temperature from the resistance and also find the heat loss from the filament in watts. I wanted to see if anything abnormal happened when the temperature was raised to the extremes which were only possible with tungsten.

The results greatly interested me, for they showed that the energy loss through the gas, which increased in proportion to the square of the temperature up to about 1,800° K, increased at a much higher rate above that, until at the highest temperatures the energy varied in proportion to about the fifth power of the temperature. This result could be explained if the hydrogen at high temperatures were dissociated into atoms. The diffusion of the hydrogen atoms from the filament, and their recombination at a distance from it, would cause an enormous increase in heat conduction. After publishing these preliminary results, I was naturally much interested in getting other information in regard to the properties of these hydrogen atoms. A large number of experiments, extending over several years, were thus made in this study of atomic hydrogen. Nearly all these experiments would have seemed quite useless, or even foolish, to a man who was making a direct and logical attack on the problem of improving tungsten lamps.

When nitrogen at low pressure was introduced into a bulb containing a tungsten filament at extremely high temperatures, such as 2,800° K, the nitrogen disappeared at a rate which was independent of its pressure; in other words, here was a case of a reaction of zeroth order. This suggested that the reaction velocity was limited by the rate at which the tungsten evaporated from the filament. To check this hypothesis the rate of loss of weight of filaments at various temperatures was measured in good vacuum. This rate varied with the temperature in accordance with known thermodynamic laws, and, since the rate per unit area was independent of the size of the filament, it was concluded that the loss of weight was really due to evaporation and not to chemical action of residual gases or to electric currents that passed from the filament to the surrounding space.

A comparison of the rate of disappearance of nitrogen with the loss of weight in the filament showed that one molecule of nitrogen disappeared for every atom of tungsten that evaporated. A brown compound, WN_2, was formed which deposited on the bulb and decomposed when water vapor was introduced, forming ammonia gas.

From time to time the question kept arising—how good would a lamp be if it had a perfect vacuum? And now, from studies of the character I have described, I began to have an answer. Hydrogen, oxygen, nitrogen, carbon monoxide, and in fact every gas that I introduced, with the exception of water vapor, did not produce blackening of the lamp bulb. The serious blackening that occurred with only small amounts of water vapor depended upon a cyclic reaction in which atomic hydrogen played an essential part. The water-vapor molecules coming in contact with the hot filament produce a volatile oxide of tungsten, and the hydrogen is liberated in atomic form. The volatile oxide deposits on the bulb where it is reduced to the metallic state by the atomic hydrogen, whereas the water vapor produced returns to the filament and causes the action to be repeated indefinitely. Thus, a minute quantity of water vapor may cause a relatively enormous amount of tungsten to be carried to the bulb.

The question then arose whether the traces of water vapor, which might still exist in a well-exhausted lamp, were responsible for the blackening which so limited the life or the efficiency of many of these lamps. We made some tests in which well-made lamps were kept completely immersed in liquid air during their life, so that there could be no possibility of water vapor coming in contact with the filament. The rate of blackening, however, was exactly the same as if no liquid air had been used.

Having thus proved that the blackening of a well-made lamp was due solely to evaporation, I could conclude with certainty that the life of the lamp would not be appreciably improved even if we could produce a perfect vacuum.

Early in 1911 William Stanley, one of the pioneers in the electrical industry, felt that our company should do more fundamental work in connection with heating devices. Since I had become interested in the theory of heat losses from filaments in gases, I was glad to work along these lines, so I undertook to direct a small laboratory at Pittsfield, Massachusetts, at which I spent about two days a week. Besides studying the heat losses from plane surfaces at various temperatures, I measured the heat losses from wires of various sizes in air at different temperatures, working at first with platinum wires, and was able to develop a theory of the heat losses which enabled me to calculate the loss from a wire of any size at any temperature in any gas, assuming, however, that the gas did not dissociate at high temperatures.

Having now a definite theoretical basis on which to calculate the normal loss by convection, I was able to prove that the abnormal rate of heat loss previously observed with tungsten filaments at high temperatures in hydrogen was due to actual dissociation; in fact, I was thus able to calculate the heat of dissociation and the degree of dissociation at different temperatures.

To make sure of these conclusions, however, I wished to make measurements of the heat losses in gases which could not possibly dissociate, and therefore undertook experiments with heated tungsten wires in mercury vapor at atmospheric pressure. A little later I experimented with nitrogen to see if this gas dissociated at high temperatures, but found that it did not do so. In both these gases the filaments could be maintained at temperatures close to the melting point for a far longer time than if heated in vacuum at the same temperature. Thus the rate of evaporation was greatly decreased by the gas, many of the evaporating tungsten atoms being brought back to the filament after striking the gas molecules.

By this time I was familiar with all the harmful effects which gas can produce in contact with filaments and knew under what conditions these bad effects could be avoided. In particular, I realized the importance of avoiding even almost infinitesi-

mal traces of water vapor. Thus, when I found a marked effect of mercury vapor and nitrogen in reducing the rate of evaporation, it occurred to me that it might be possible to operate a tungsten filament in gas at atmospheric pressure and obtain a long useful life. Of course, it would be necessary to raise the temperature far above that at which the filament could be operated in vacuum in order to compensate for the serious loss in efficiency due to convection by the improved efficiency resulting from the rise in filament temperature. Whether or not the increased rate of evaporation, due to this increase in temperature, would more than offset the decrease in the rate due to the gas was a matter that could only be tested by experiment.

In connection with my studies of the heat losses from filaments of various diameters at incandescent temperatures, I had found that the heat loss increased only very slowly with the diameter, so that the loss per unit area from a small filament was enormously greater than from a large filament. Calculations showed that it was hopeless to get practical lamps with filaments in nitrogen, if these filaments were of very small diameter. For example, a filament 1 mil in diameter, which corresponds to an ordinary 25-watt lamp, if run in nitrogen at atmospheric pressure, would consume 4.8 watts per candle at a temperature of 2,400° K, which would give 1 watt per candle with a filament in vacuum. This great loss in efficiency is due to the cooling effect of gas. To bring back the efficiency of the gas-filled lamp to that of the vacuum lamp, it would be necessary to raise the temperature from 2,400° to 3,000° K, which would have caused a 2,000-fold increase in the rate of evaporation, and such an increase could certainly not be compensated for by the effect of the gas in retarding the evaporation.

With filaments of much larger diameter, however, the effect of the gas in decreasing the efficiency was not nearly so marked. We therefore constructed lamps having filaments of large diameter in the form of a single loop and filled these lamps with nitrogen at atmospheric pressure. We ran these lamps with a filament temperature so high that, in spite of the gas, the efficiency corresponded to about 0.8 watt per candle instead of the usual 1 watt per candle at which we tested our vacuum lamps. We were disappointed to find that these lamps blackened much more rapidly than vacuum lamps of similar efficiency, so that the total useful life of the lamp was short.

This result, which is what most lamp engineers would have expected, seemed to indicate that the rise in temperature necessary to offset the heat losses by the gas increased the evaporation by more than the amount of the reduction in evaporation due to the gas. If I had not previously become so familiar with the behavior of various gases, this discouraging result might easily have stopped further experimenting in this direction. However, I noticed that the bulb had blackened during the short life of the lamp, whereas from my knowledge of the interaction of tungsten and nitrogen I had expected a deposit of a clear brown color. I felt that the black deposit, therefore, could mean only one thing, water vapor, notwithstanding the fact that to avoid this water vapor we had taken precautions which were greater, I believe, than had ever been used before for the preparation of moisture-free gases and glass surfaces. We were thus led to take still greater precautions and use still larger bulbs, so that the glass surfaces could not become overheated by the convection currents in the gas that rose from the filament. We were then soon able to make lamps having a life of over 1,000 hours, with an efficiency about 30–40 per cent better than could have been obtained with filaments in vacuum.

As I look back upon these experiments I think that we were very fortunate at that time in not having had at our disposal a supply of argon gas. From theoretical reasons I had concluded that argon should be better than nitrogen, and if I had had argon I should probably have tried it first. If these lamps had blackened because of traces of water vapor, I would naturally have attributed this to the increase in evaporation caused by the high temperature, and would have had no reason for suspecting that water vapor was the cause of the trouble, for, of course, in argon a brown deposit would not be expected in any case.

The lamps that we were able to make in this way, with an improved efficiency, were limited to those which took a current of 5 amperes or more, so that the method was not applicable for 110-volt lamps with less than 500 watts. Some time later, however, it occurred to me that the benefits derived from the large diameter of the filament could be obtained with one of smaller diameter by coiling the filament in the form of a helix, bringing the turns of the helix very close together. In this way, and by the use of improved tungsten filaments that do not sag so readily at high temperatures, and by using argon instead of nitrogen, it has gradually been possible to construct gas-filled lamps which are better than vacuum lamps down to about 40 or 50 watts. These smaller lamps, although not much better in efficiency than the vacuum lamp, have the advantage of giving a much whiter light. In the case

of the larger lamps, the use of the gas filling, together with the special construction of the lamp, more than doubles the efficiency.

The invention of the gas-filled lamp is thus nearly a direct result of experiments made for the purpose of studying atomic hydrogen. I had no other object in view when I first heated tungsten filaments in gases at atmospheric pressure. Even at the time that I made these experiments at higher pressures, they would have seemed to me useless if my prime object had been to improve the tungsten lamp.

I hope I have made clear the important role that properly encouraged scientific curiosity can have in industrial research. The illustration that I have given is not at all exceptional. I could have given any one of several others equally well. Many industrial laboratories have followed Dr. Whitney's lead, in devoting a fairly large fraction of their activities to these rather purely scientific researches. Certain men, at least, are not expected to be responsible for practical applications, but are freely allowed to make fundamental scientific investigations. The type of man who does this work best can usually only be attracted to those industrial laboratories that have adopted this policy.

However, I do not believe that this second method of research is growing in popularity solely because it is found to be profitable. I feel rather that most of our leaders in industrial research are eager to adopt this method, in so far as economic factors may permit, because they realize the debt that modern industry owes to the pure science of the past and because the modern conception of service and the growing *esprit de corps* of American industry help make them glad of any opportunity to contribute to scientific knowledge. I know personally that such motives as these have guided Dr. Whitney in the leadership he has taken.

I believe in the near future there will be a much increased demand for men with scientific training who are capable of doing more independent thinking.

Better Education Needed

Our schools and universities devote so much effort in imparting information to students that they almost neglect the far more important function of teaching the student how to get for himself what knowledge of any subject he may need. Even in grammar school children are crammed with more information on arbitrarily selected subjects than even the average well-educated adult can retain. Of course students should be taught the fundamental principles of mathematics, and of various sciences as well as of other subjects, but much of the knowledge of data upon which these principles depend, and necessary information, should be obtained by the efforts of the student through experimentation and individual reading.

In looking back on my own school and college days, it seems to me that the things of most value were learned spontaneously through interest aroused by a good teacher, whereas the required work was usually comparatively uninteresting. The university student should have leisure for some independent work and opportunities for continuing his interest in hobbies of various kinds which he should have had long before he entered college. I realize that it is difficult so to arouse the student's interest that he will spend the added leisure in these ways rather than in spending still more in the bleachers, cheering the football team in their practice games, but a well-planned effort is worth while.

The importance of arousing even a young boy's interest in independent work can hardly be overemphasized. My real interest in science was derived from my brother Arthur, who encouraged me to have a workshop at the age of nine, and later a laboratory when I was only twelve.

I can illustrate my father's influence in stimulating independence by the following incident: When I was twelve I climbed one or two Swiss mountains of moderate height with my older brother, Arthur. Soon after, Arthur had to go to Heidelberg to arrange for his studies, leaving me with my mother and younger brother at a hotel in the Rhone Valley. I had become so enthusiastic over mountain climbing that I wished to climb everything in sight, but the dangers were such that my mother did not dare let me go alone. When my father arrived for a weekend visit from Paris, he consented to allow me to climb alone any mountain I liked if I would promise to do it in accord with the following three rules: (1) I must stay on a distinct trail; (2) I must use the same trail going and returning; (3) I must make certain of returning at six o'clock by allowing as much time for decending as for ascending. Before these rules went into effect, however, I had to prove that I could and would make such sketches, maps, and notes of the trails used for the ascent that I could always return by the same route. I thus climbed several mountains about 7,000 feet high, often requiring several days of repeated effort before I could discover a route that led to the top. Perhaps it is this experience which makes me even today always wish to find my own way rather than be told.

Until I was fourteen I always hated school and

did poorly at it. At a small boarding school in the suburbs of Paris, however, being an American and having a friend who was influential with the head of the school, I was freed from much of the absurdly rigorous discipline to which the French boys were subjected. Thus, I could spend time alone in the school laboratory and was encouraged by one of the teachers to learn to use logarithms and solve problems in trigonometry, subjects not required by the curriculum.

I have been fortunate in having had many wonderful teachers. Three of them have been recipients of the Perkin Medal. Whitaker and Chandler were my teachers at Columbia and Whitney during the last eighteen years. Professor R. S. Woodward, at Columbia, in connection with his courses in mechanics, was extremely stimulating and encouraged me to choose and solve my own problems for classwork instead of those required in the regular course.

I should like to see spontaneous work of this kind take a much more prominent part in our educational system—at least for students who have more than average ability.

The Value of Hobbies

Very great benefit may be derived from hobbies. Probably each person should have several of them. Recently I met a small boy, only six years old, who had an overpowering, wide-eyed enthusiasm for collecting insects. He weighed each one of them within a milligram, and then, after desiccating them thoroughly over calcium chloride, weighed them again. Many elaborate notes and even correspondence resulted. I am afraid our universities, with their dormitories and other standardizations, tend to discourage such wholesome individual activities.

After talking of hobbies, I cannot resist the temptation to tell something about my own. Perhaps my most deeply rooted hobby is to understand the mechanism of simple and familiar natural phenomena. I will give only two illustrations, but these, I hope, will show how easy it is to find around us simple phenomena that are not well understood.

Every chemist knows that after he stirs a liquid in a beaker having a precipitate in the bottom, the precipitate collects near the center. Probably few know why this is so. It is not due to the slower velocity of rotation near the center, nor to the slower motion with respect to the glass. This is proved by the fact that if you put the beaker, with the precipitate in suspension in the liquid, upon a rotating table, the precipitate will collect in a ring as far from the center as possible, although the relative angular motion of the beaker and its contents is the same as before. A little study proves that the phenomena are due to unbalanced centrifugal forces. For example, when the liquid is stirred to set it in rotation, centrifugal force produces a greater hydrostatic pressure near the walls of the beaker. But the liquid very close to the bottom surface of the beaker, because of friction, cannot rotate so fast, and therefore the centrifugal force is not so great and does not counteract the radial hydrostatic pressure difference existing in the upper layers. The liquid in contact with the glass bottom is thus forced inwards and carries the precipitate with it.

The phenomena connected with the formation and the disappearance of ice in a large lake, such as Lake George, have interested me for years. One clear night at the end of December, when the water of a large bay was at a uniform temperature of not over 0.2° C and the air temperature was −22° C, ice, which formed slowly at some places on the shore, melted in a couple of minutes when pushed out a few meters from the shore. There was no wind in the bay, but a slight breeze over the central part of the lake caused a very slow circulation of water in the bay with a velocity of perhaps 1 or 2 cm per second.

In contrast with this consider the phenomena observed one clear afternoon of the following April. The body of the lake was still covered with ice, which was about 20 cm thick, but close to the shore there were places where the ice had melted back for a distance of 5 meters or more. Although the air temperature was +3° C and the water 10 cm below the surface was at +2.5° C, ice crystals about 50 cm long formed in these pools in less than half an hour. After considerable analysis I believe I can explain this apparent paradox by the stability in the stratification of the water in April caused by the denser underlying warm water which had been heated by the sun. With this stability, which prevented vertical convection, the surface water could freeze because of the radiation into the clear sky. But in December the water temperature was so uniform that the differences of density were not sufficient to prevent vertical circulation, and thus the surface could not cool to the freezing point. It appears, then, that a pool of water at +1° C, exposed to cold air with a slight wind, can be made to freeze more rapidly if the water is heated from the bottom. Sometime I want to try this as an experiment.

All hobbies, however, stimulate individual action, and many develop wholesome curiosity. The child should acquire them early, and our educational system should foster them.

The Gifts of Hybridity

W. GORDON WHALEY

Formerly with the Department of Agriculture's Bureau of Plant Industry at Beltsville, Maryland, Dr. Whaley (Ph.D., Columbia, 1939) left government service in 1946 to join the Plant Science group at the University of Texas, where he is currently professor of botany, chairman of the Botanical Laboratories, and director of the Plant Research Institute. He is the author of numerous scientific papers (cf. Sci. Mon., *Feb. 1946, 21–31*) *and of* Biology for Everyone (*Halcyon House, 1948*). *His recent investigations have shown the greater phosphorus absorptive capacity of corn hybrids over their inbred parents.*

IN THE year 1932 corn was planted on 113,024,000 acres of United States farm land. The total yield for that year was 2,930,352,000 bushels, an average of 25.9 bushels per acre. In the year 1946, 3,287,927,000 bushels were harvested from plantings on 90,027,000 acres, representing a per acre yield of 36.5 bushels. The difference in yield was due in greatest measure to the use of hybrid corn on a large scale. The production of 36 bushels to the acre instead of 26 represents nothing short of a revolution. The importance of the revolution extends even further than the increased yield figures indicate, for it has freed approximately 23,000,000 acres of land for the growing of other crops or for inclusion in a rotation and conservation scheme to provide a hedge against soil fertility exhaustion.

These developments suggest startling potentialities for other crops, and they may be a consideration pointing the way out of the dilemma of increasing populations and decreasingly fertile farm lands with which most of the Temperate Zone countries of the world are faced.

The superiority of hybrid corn has its basis in a little-understood phenomenon known to biologists as heterosis. After some preliminary observations, we shall consider the various hypotheses as to the nature of heterosis. Whatever may be involved, heterosis gives to hybrids a developmental vigor which makes them larger, higher-yielding, improves the quality of their products, or otherwise renders them more desirable than their parents. The occurrence of this hybrid advantage, generally referred to as hybrid vigor, provides one of the most intriguing of biological puzzles.

Our discussion of it must be prefaced with a note about the use of the word "hybrid." In a narrow sense, a hybrid is an offspring of two different species. In the broader sense in which we shall use it here, hybrids are the offspring of genetically different types. These may be species, varieties, or lines, types which differ too little to be called separate varieties.

The discovery that among both plants and animals certain cross-fertilizations result in the production of progeny more vigorous than either parent was made long ago. There are records of plant hybridizers taking practical advantage of hybrid vigor as early as the last quarter of the eighteenth century. The value of the mule, which is often, though perhaps not correctly, cited as a classic example of hybrid vigor, was recognized by the contemporaries of Moses. Through the years it has been learned that hybrid vigor is the result of interbreeding within plant genera as diverse as corn and oaks and within animal genera as different as fruit flies, fish, cattle, and man.

Any phenomenon as common and of as great potential value as hybrid vigor naturally attracts attempts at explanation. Much effort has been expended by a great number of biologists to determine the cause of hybrid progeny superiority, partly in the hope that, if the answer can be found, widespread improvement of domesticated plants and animals will be possible. The hypotheses put forward on the basis of research and speculation have been many and varied. None is entirely satisfactory, a fact which leads to the suggestion that there may be several different causes.

The Basic Problem

There is a vast literature on the subject of hybrid vigor. Part is devoted to the usefulness of hybrid vigor as a manageable biological occurrence, and part is devoted to the opportunities that

hybrid vigor presents for approaching certain complex questions of inheritance and development. When one examines the results of research, it becomes apparent that there is no single definition of what constitutes hybrid vigor. Different plant types exhibit hybrid vigor in different ways. For example: In corn, in which greatest practical use has been made of the characteristic, the hybrid advantages are generally recorded in terms of total yield, weight, length of the ears, number of grains per row on the ear, and number of nodes per plant. There appear to be "quality" factors involved also, but these are much more difficult to evaluate. Among them are likely to be differences in chemical composition, making for differences in nutritive value. Unfortunately, we do not know enough about such "qualitative differences" to discuss them. It has been noted by many workers that it is the size of the parts rather than the number which is affected in corn. This point is rather significant. In the tomato, on the other hand, the effects are mostly in increasing the number of parts rather than the size of the individual parts.

The differences in manifestation of hybrid vigor in corn and tomato are related to differences in growth habit. Corn is a determinate plant in which, after a certain number of internodes have been formed, the growing shoot apex matures as a flower structure. Tomato is an indeterminate plant which forms an indefinite number of lateral branches and bears lateral rather than terminal flowers. Apparently, in determinate plants like corn, added vigor increases the magnitude of the parts, the number of which is more or less fixed by the genetic pattern. In indeterminate plants like tomato, added vigor results in faster or longer-continued production of the new parts. There may be an important clue as to the nature of hybrid vigor in these differences in expression, for they seem to suggest that we are not dealing so much with a change in the basic genetic pattern as with an increase in the efficiency of growth and development. If this is true, it lays emphasis upon the need for basic plant research in our efforts to understand hybrid vigor and to extend the benefits of hybridity to a great number of crop plants. This concept becomes more tenable when we examine the nature of the characteristics associated with hybrid vigor in animals. Here the offspring may not necessarily have a size advantage over their parents. Rather, their advantages are in terms of more efficient metabolism, later onset of senescence, less rapid progress of senescence, greater reproductive capacity, or similar traits.

Hybrid vigor in corn presents the best example for consideration. Corn has been grown in the Western Hemisphere for hundreds of years. It has been subject to so much artificial selection that it was long ago modified to the point where it can now exist only in cultivation. The fact that a plant with such a long history of use could be so greatly improved in the short space of a few decades is pertinently suggestive for our handling of other plants and animals with much shorter histories of domestication. Corn has certain characteristics of growth and reproduction which make it particularly favorable for the manifestation of hybrid vigor, and examination of the development and utilization of hybrid vigor in corn brings to light facts which promise much for the improvement of other crops.

Despite much research and a great deal of speculation, we still have no certain knowledge as to the ancestry of the modern cultivated type of corn. It is clear, however, that corn early became distributed over a fairly wide range. It was grown by most of the American Indians. As a basic Indian food crop, the distribution of corn was over a number of areas more or less sharply isolated from one another. In each of these there appeared much variation, some of which has long since disappeared. Some of it has been preserved in the germ plasm of corn. Because the corn-growing areas were more or less isolated from one another and represented different environmental conditions, different sets of variants have been preserved in the differet areas. We must look upon the species *Zea mays* L. as containing a rather spectacular range of character variation or, in genetic terms, as having a rich diversity of genes. Had corn been grown differently throughout the centuries, a few selected types might have been preserved at the expense of the elimination of all the others, but as grown by the Indians and their white successors to the land, corn was handled, until fairly recently, in such a manner as almost to preclude the production of any pure genetic stocks. It is an ancient observation that corn tends to gain in vigor as diverse kinds are combined. Both the Indians and the early white settlers frequently made a practice of planting blue grains and red or yellow grains and white grains together, or making other combinations which tended to result in a mixing of kinds, because the yields in such mixed plantings were higher than those obtained when a single type of corn was planted by itself.

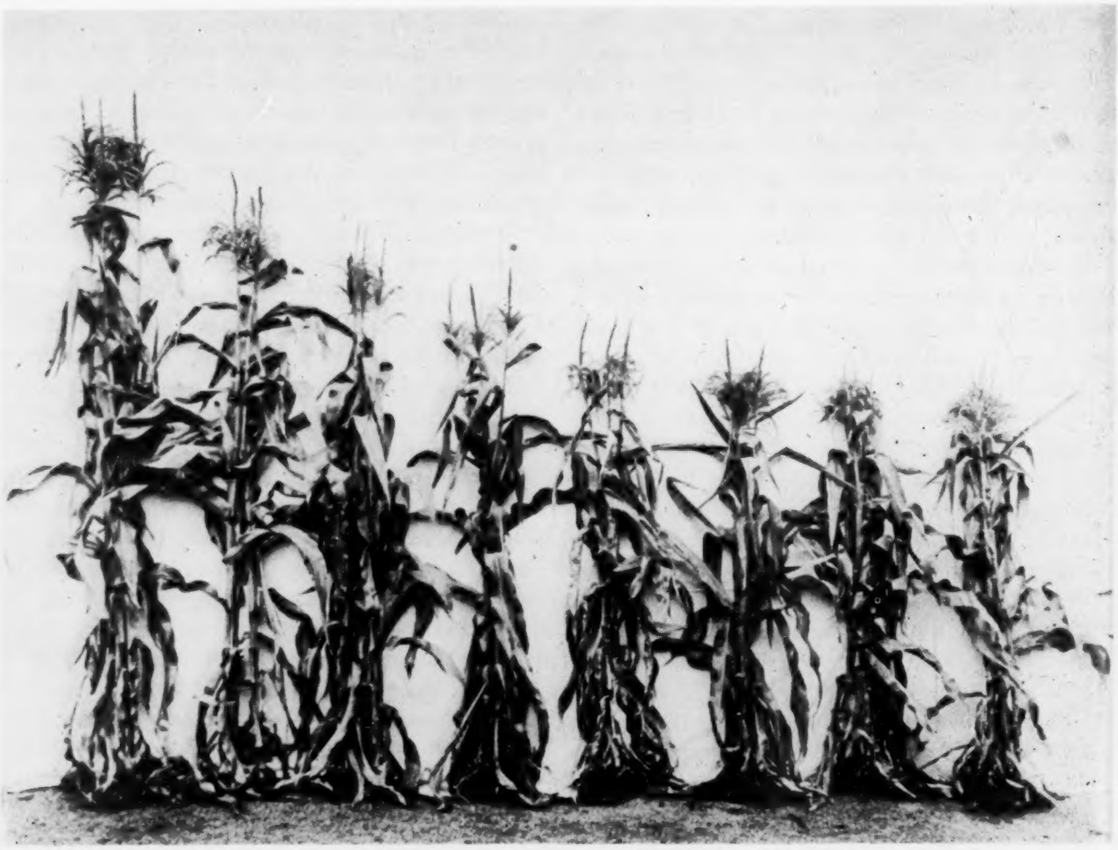
With the advent of scientific plant breeding came efforts to produce true breeding stocks of plants. It was early discovered than when corn is self-fertilized there results a segregation of lines, often with quite different characteristics. Repeated self-fertilization of these lines ultimately produces stable or nearly stable stocks, although generally a great number of the lines are lost during the inbreeding operations. It is a universal observation that inbreeding in corn to produce pure lines is attended by marked degeneration. This degeneration is frequently so severe that a large proportion of the lines are lost and only the less degenerate ones survive.

When different surviving inbred lines of corn are crossed they most frequently produce hybrid progeny which not only exceed their parents in vigor, but generally are more vigorous than the stocks from which the inbred lines were originally derived.

There is a rough, though not infallible, rule of thumb that the amount of hybrid vigor is a reflection of the amount of genetic difference between the two lines which are crossed. The combination of inbred lines of widely separated origin generally gives more vigorous hybrids than the combination of lines of common origin. This picture is complicated, however, by the fact that certain lines seem to combine better than others.

Hybrid corn is valued first of all for its relative vigor, which not only makes for greater yield, but which also, apparently, provides better adaptability to environmental conditions. Too, being a genetically uniform stock, hybrid corn grows and develops much more uniformly than do randomly pollinated stocks. This latter fact is often more important than the first one in regions in which corn farming is highly mechanized, for the mechanical harvesters can handle successfully only uniform crops.

Recently the techniques of hybridization have been extended to produce what are known as double hybrids, or sometimes "double-crossed" corn. Inbred A is crossed with inbred B to produce the hybrid AB. Inbred C is crossed with inbred D to produce the hybrid CD. The two hy-



An example of the degeneration that accompanies inbreeding in corn. The plants represent an F_1 hybrid and seven subsequent generations. Note that the amount of degeneration tends to decrease in the later generations.

brids are then crossed to produce the double hybrid ABCD. In many instances such double hybrids have certain added advantages. Occasionally they are more vigorous than their immediate hybrid parents, but more usually their advantages are found in somewhat greater uniformity of growth and development and a little wider range of adaptability, probably brought about by the combination of four rather than two selected germ plasms.

The hybridization of corn has now reached practically an assembly-line stage. The production of hybrid corn seed is a big business, with competitive aspects which will keep alive attempts to produce new and better hybrids. Like any strain of plants, the efficiency of a hybrid is dependent upon a proper balance of inherited characteristics, the soil, climate, and other factors which constitute the environment. A hybrid which will grow well and produce large yields in one locality may be distinctly inferior in another. This feature of hybrid corn production gives the problem some local flavor and makes it necessary to study the behavior of individual hybrids under given sets of conditions before their adaptability and usefulness can be determined.

The Genetic Basis of Hybrid Vigor

Presumably our ability to produce better hybrids would be greatly enhanced by a knowledge of the genetic basis of heterosis and the physiological and developmental mechanisms responsible for the hybrid advantages. The association of added vigor with hybridity was early attributed (Jost, 1907) to the union of dissimilar elements at the time of fertilization. This general concept depended for its validity upon a sort of extragenetic stimulation resulting from the association of egg protoplasm of one type with sperm protoplasm of another type.

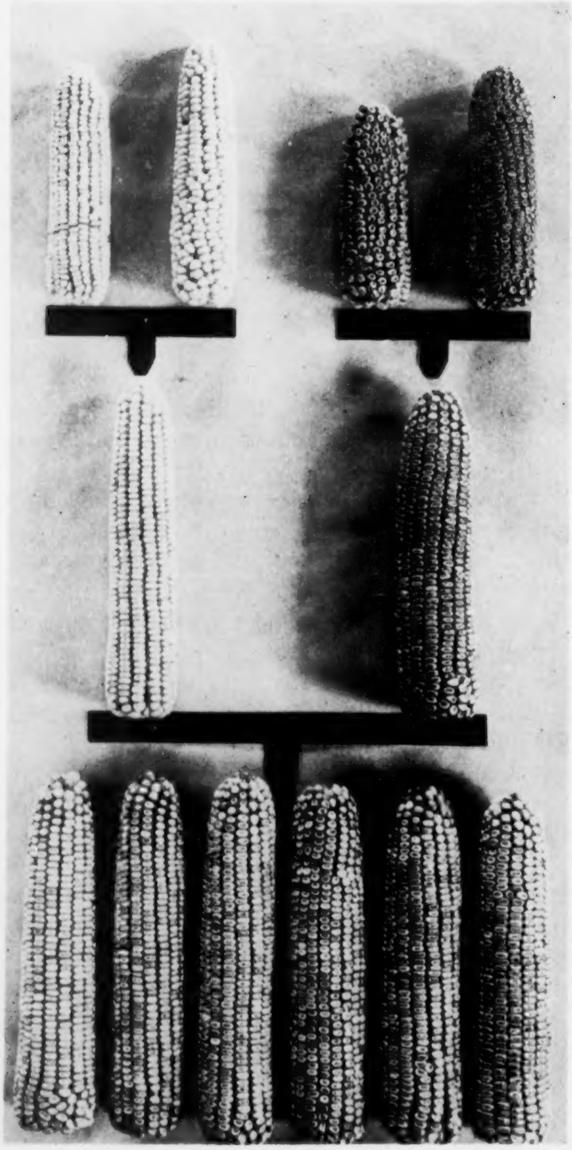
With the rise of genetics, it became apparent that such phenomena as hybrid vigor had a more likely explanation in terms of Mendelism. The hypothesis generally accepted in explanation of hybrid vigor was put forth by D. F. Jones, of the Connecticut Agricultural Experiment Station, in 1917. Jones' hypothesis was an outgrowth of the work of G. H. Shull, E. M. East, and H. K. Hayes, all of whom did pioneer work in investigating inheritance in corn. Shull was presented with the Marcellus Hartley medal, commonly known as the Public Welfare medal, by the National Academy of Sciences on April 26, 1949, in recognition of his work on hybrid vigor in corn.

Briefly, Jones' hypothesis, generally referred to as the "dominance-of-linked-genes hypothesis," states that hybrid vigor results from combining in a hybrid one set of favorable characteristics for which one of the inbreds contains dominant genes and another set of favorable characteristics, the development of which is controlled by a different set of dominant genes in the other inbred. The assumption that hybrid vigor results from the action of a large number of favorable dominant genes, many of them linked by virtue of being on the same chromosome, made possible an explanation in line with the newly accumulated genetic knowledge and in accord with the known facts pertaining to hybrid vigor. In 1936 East postulated that perhaps different mutations at the same gene locus might also be involved in the production of hybrid vigor. Although the hypothesis which East put forward did not find much acceptance at the time, it has become increasingly significant because it sets forth the idea that when a gene mutates its particular function may be significantly altered.

Still more recently there have been recorded instances in which hybrid vigor seems to have its basis in single gene differences (Singleton, 1943; Quinby and Karper, 1946).

We are still unable to settle on one explanation of the genetic background of hybrid vigor. Indeed, it becomes increasingly clear that there may be a number of different genetic pheomena involved. We can, however, discuss some of the factors responsible for the vigor and other advantages displayed by hybrids. When the science of genetics dealt almost exclusively with the study of mature characteristics as related to their genetic bases, it was common to talk in terms of quantitative characteristics and to assume that given genes added, say, two centimeters of length to the ears of corn or two rows of kernels or six kernels in a row, or some similar unit. Continued study of plant and animal genetics in conjunction with the development and functioning of the plants and animals has led us to another interpretation of such differences. We now think of an inherited pattern of development and an inherited pattern of metabolism which, subject to modification by factors in the environment, unfolds the pattern of development.

There is some analogy between our present concept of the roles of heredity and environment and a musician playing from a score of music. The score is fixed, but the actual playing can vary, for the musician may play well one day and poorly the next, depending, in part at least, upon environmental factors. The genetic pattern of an



Ears from four inbred lines, the two single-cross hybrids and the double-cross hybrids.

organism is fixed at the time of fertilization, but its ultimate expression depends upon the extent to which it is unfolded and how it is modified in the course of its unfolding in a particular environment.

If a number of cases of hybrid vigor in plants are studied, they will usually fall into several categories. Some of the hybrids will be larger and will yield more because they grow longer than the inbreds. Others grow at a faster rate during the so-called grand period of growth. Still others grow much more rapidly during the early phases of development.

In respect to greater early growth, there has been much discussion of the significance of seed and embryo size in giving advantage to hybrids. It was postulated by Ashby (1930) that an advantage in corn seed size is responsible for the mature advantage of the hybrid. Later Ashby (1932, 1936, 1937) laid emphasis upon the significance of greater embryo size and then finally upon greater mass of meristematic tissue within the embryo. Actually, although these factors may be involved in certain instances of hybrid vigor, we now know that there are many cases in which very vigorous hybrids grow from seeds no larger than, or not as large as, those of the parents, and that the embryos and the mass of meristematic capital may both be smaller in the hybrids.

Physiological Factors and Hybrid Vigor

The most frequently noted hybrid advantage seems to be more efficient metabolism during and/or immediately after germination. The hybrid plants get off to a better start and then settle down to grow at approximately the same rate as the faster-growing of the parents. Having a head start, the hybrid plants hold their advantage even though they have the same rate of growth as the parent, and at maturity the hybrids are larger, higher-yielding plants.

The existence of this kind of advantage puts study of the hybrid vigor problem into the hands of the physiologists. Although investigation of the physiological phase has been limited, there are some suggestions as to the kind of factors that may be involved. Robbins' work (1941a and b) at the New York Botanical Garden, as well as our University of Texas studies, indicates that there are growth substance differences involved in some cases. If one of the inbreds has the ability to synthesize growth substance X in satisfactory amounts but lacks the ability to synthesize growth substance Y, and the other inbred presents the reciprocal situation of being able to synthesize substance Y but not substance X, there is a mechanism for the combination in the hybrid of abilities to synthesize both X and Y. If these substances have important roles in the physiology of early development, they will provide a basis for the expression of hybrid vigor. This particular mechanism has been studied in some detail in cultures of excised roots of tomato where hybrid vigor is often markedly manifest. In such cultures thiamin, pyridoxine, and niacin, members of the B vitamin group, appear to be involved in controlling the rate and extent of growth. It is not suggested that

these particular substances are controlling factors in the case of whole plants, for green tissues seem to be able to synthesize all of them satisfactorily. The investigations do indicate, however, a possible type of mechanism and its action.

A different kind of factor is indicated by studies which show that vigorous hybrids often have advantages in the absorption, and perhaps the distribution, of certain minerals. Whether such advantages involve membrane permeability or more active physiological mechanisms or differences in the structure and organization of the roots has not yet been determined. It is clear, however, that where a hybrid has heightened ability to absorb and translocate a substance such as phosphorus, which plays a vital part in early growth, the plant has distinct advantages over less efficient phosphorus-absorbing lines.

Reproduction and Hybrid Vigor

Jones' accepted hypothesis as to the genetic basis of heterosis postulates the sorting out of so-called dominant favorable genes during the process of inbreeding. This means that one inbred will have a certain group of dominant favorable genes, whereas another inbred will have a different group of dominant favorable genes. When these two inbreds are crossed, the hybrid produced is a better plant because it contains both groups of dominant favorable genes. Although there are certain technical objections to this hypothesis and although, at least with reference to some of the occurrences of hybrid vigor, it lays the emphasis in the wrong place, it nonetheless states the general concept of what must be the basis of heterosis.

If we select an open-pollinated population of corn of the sort grown by our grandfathers, self-pollinate a number of the plants, and then grow the progeny of these self-pollinated plants, obvious degeneration of the stock takes place. Genetically, the basis of this degeneration is to be found in the nature of evolutionary changes. It is a well-established fact that from time to time the genes of organisms undergo changes, commonly called mutations. We do not know the nature of these changes, nor do we know much about their causes. We do have ample evidence that the frequency of some of them can be increased by certain experimental procedures, such as exposure of cells to various kinds of radiations, to temperature modifications, and to certain gases. It is, however, obvious that when the gene mutates, the character whose expression that particular gene controls is also modified. The evidence suggests that at least insofar as the organism's adaptation to a particular environment is concerned, some of these mutations are in a favorable direction, others in an unfavorable direction. Presumably selection operates to make the fixation of the favorable ones in the organism's genetic constitution a certainty. The fate of the unfavorable mutations is directly dependent upon their nature, the type of reproduction of the organisms in which they occur, the size of the breeding populations, and other factors.

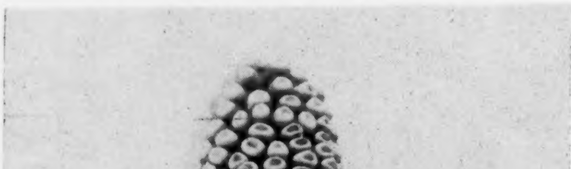
It is with these unfavorable mutations that the majority of experiments on heredity have been concerned, and it is the unfavorable mutations that must be studied carefully in relation to heterosis. A particular unfavorable mutation may be either dominant or recessive to the state of the gene from which the mutation occurred. Dominant unfavorable mutations tend, if severe enough, or if accumulated in great enough numbers, to eliminate the organism. Recessive unfavorable mutations, however, may persist in organisms for long periods and be accumulated in great numbers be-

F_1 hybrid plants with the dwarf parent strains shown in the rows on either side of the hybrids.

cause their effects are wholly or partially covered up by their dominant counterparts. The studies of Dobzhansky and his co-workers (1938, 1942) on natural populations of the fruit fly, *Drosophila pseudoobscura* Frolowa, reveal that in these populations there are large numbers of more or less unfavorable recessive mutations which can be uncovered by proper experimental techniques and which certainly have great effects upon the character of the natural populations. It is with these unfavorable recessive mutations that we are primarily concerned.

First, we must consider the matter of degree of unfavorableness. Genetic studies of many organisms indicate clearly that the unfavorableness of mutations may range all the way from a very slight degree to an extreme in which the effects of a single deleterious gene bring about early death of the organism. Arbitrarily, these unfavorable genes may be classified as weak developmental modifiers, strong developmental modifiers, semilethals, or lethals, depending upon their degree of deleteriousness.

Weak developmental modifiers are those mutations whose effects upon development are deleterious but relatively slight. A single such mutation might produce no effect detectable in ordinary experimentation. The effect of an accumulation of a number of such modifiers would be determined by the number and the relative deleteriousness of the individual mutations. The strong developmental modifiers differ from the weak ones in degree. The semilethals are mutations which have a lethal effect either in the presence of certain other genes or under particular environmental conditions. It should be emphasized that there is, of course, no line between any one of these classes and another, that the designation is purely arbitrary, and that there are not only intergradations from one group to another, but that the effects of a specific mutation might make it classifiable in one group under a given set of conditions and in another group under a different set of conditions.


An F_1 hybrid ear and the ears of its two inbred parents.

Since the effect of the lethals is one of complete elimination this class of mutations is of no interest in a heterosis study. The effects of the other mutations must be considered in relation to reproduction and population size.

With respect to reproduction, plants may be divided into three more or less distinct groups: those normally cross-pollinated, those normally self-pollinated, and those in which both cross- and self-pollination normally take place. The type of pollination is, in part, a determiner of breeding population size and hence controls the accumulation of unfavorable recessive mutations.

In cross-pollinated plants the accumulation of semilethals and both powerful and weak deleterious developmental modifiers may be anticipated. The rate at which such modifiers are accumulated is conditioned by mutation rate, the character which the gene controls, and the number of modifiers already present.

If a breeding population is small the unfavorable recessive genes will be brought together in fertilizations frequently, and, if there are enough of them, plants which contain them will be eliminated. If a breeding population is large, combinations of unfavorable mutations will be relatively less frequent. Large populations may accumulate many such genes, and the genes accumulated may have a relatively greater degree of deleteriousness without any appreciable damage to the population. Small populations can accumulate fewer deleterious genes, and the degree of deleteriousness must be less than in large populations.

If hybrid vigor is manifest as the result of covering up these unfavorable recessive genes, there should be differences in the amount of hybrid

Good stand of hybrid corn.

vigor found in organisms with different-size breeding populations. Actually such differences exist. Plants that are completely self-pollinated have, for practical purposes, a breeding population of but a single individual. The garden pea with which Mendel worked is an example. Such plants rarely exhibit much hybrid vigor. In plants like the tomato, where pollination is not completely by selfing, the hybrids exhibit relatively more vigor. In plants like corn, which are wind-pollinated and grown over large acreages—that is, where the breeding population is large and where isolation has been at work for a long time—the amount of hybrid vigor is great. This situation suggests two good possibilities for plant-breeding operations. First, hybrid vigor can undoubtedly be utilized in a great many large-population-size, cross-pollinated plants in which, up to now, it has been of little interest. Second, self-pollinated and other small-population-size plants can be handled in such a way as to establish strains in which there is a greater accumulation of factors that will lead to the production of vigorous hybrids.

There are other explanations of at least some of the cases of hybrid vigor, but this covering up of unfavorable mutations which have been accumulated in the course of evolution is most likely responsible for the majority of cases. Recognition of this fact alters the generally accepted notion of hybrid vigor in one important sense. It has been assumed that heterotic hybrids had growth and yield values somewhat above par. Actually, this is not so. Such hybrids represent, instead, a closer approach to par value than the inbreds which have had their growth and yield reduced by an accumulation and unmasking of unfavorable genes. We ought to think of the vigorous hybrids as representing recombinations from which the manifest effects of accumulated unfavorable factors have been removed.

There have been a number of attempts to ex-

plain heterosis in terms of heterozygosity, the combination in the hybrid of dominant genes from one inbred and their recessive counterparts from the other, without reference to the covering up of deleterious recessives or positive favorable action of the dominants. Promulgation of Jones' hypothesis was an effort to get away from the assumption that the heterozygous condition could have any advantage over the homozygous condition. For many years it was thought that a dominant gene controlled one set of functions and characters and that its recessive counterpart controlled the same set, but in an opposing manner. As genetics has become a more critical science, several exceptions to this theory have been uncovered. It is now clear that when a gene mutates it may assume a different set of functions. If the gene *M* controls a certain set of processes and reactions and it mutates to *m*, the mutated gene *m* may control a slightly different set of processes and reactions. Combination in an individual of two units of the recessive mutation *mm* may be distinctly unfavorable as compared with the homozygous dominant condition *MM*, but the combination of *M* and *m* may result in a process and reaction summation with a favorable effect on growth and development. It is not true on the basis of the existing evidence that no advantage can be derived from heterozygosity, except advantage due to the presence of one dominant gene. The end effects of the association of two different states of the gene may be distinctly different from the association of similar states. This being the case, our concepts of dominance and recessiveness are more or less void with respect to the development of hybrid vigor as the result of such combinations. Possibly the phenomenon may be ascribed to heterozygosity as opposed to homozygosity.

To whichever of the genetic bases heterosis is related, it has its roots in the changes wrought by evolution. The phenomenon presents an attractive field for investigation of the part played by specific factors in evolutionary change. Our studies of heterosis contribute to several fields of knowledge. In addition, the practical usefulness of these effects of hybridity is tremendous. Can we but learn enough of their nature and development we can make available the gifts of hybridity in many important crop plants and domestic animals. The bequest might go far toward meeting the food and energy demands which increasing populations are imposing upon the world.

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All photographs by courtesy of the Connecticut Agricultural Experiment Station.

The Methods of Science: What Are They? Can They Be Taught?

ERNEST NAGEL

A native of Czechoslovakia, Dr. Nagel took his Ph.D. in philosophy at Columbia University in 1931, and has been a member of the faculty of his alma mater since that time. His article is based on an address given in a Conference on "The Place of Science in General Education" at the Harvard Summer School in July 1949.

ALTHOUGH there is an obvious presumption on the part of one who is not a trained scientist in discussing the place of science in general education, I shall not apologize for my daring. The only ready excuse I can offer is that perhaps just because I am an outsider to the ranks of professional scientists, there may be a small chance that I can note features of the over-all landscape that do not fall within the focus of attention of those whose normal preoccupation is with but a fragment of it.

I

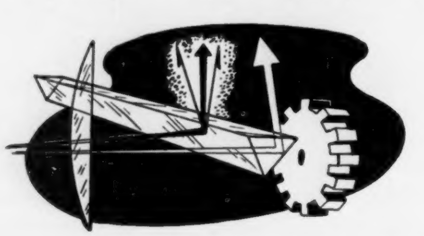
My first remark must be addressed to the formulation of our theme. Are there several methods of science, or is the use of the plural noun the consequence of myopic attention to details to the neglect of a common pattern in a diversity of materials? Although I recognize that the question may be only a verbal one, it is nevertheless worth making explicit, so far as space permits, just what we are prepared to understand by the word "method." It is obvious that any two inquiries, whether in remotely related or in intimately dependent branches of science, will exhibit identifiable and important differences; and it is possible to argue from these differences to the conclusion that in pursuing their objectives the sciences employ a variety of methods. But it is equally clear that any two inquiries will possess some common features, so that by insisting on these to the exclusion of the manifest differences one can without difficulty maintain the unitary character of scientific method. Some distinctions are obviously needed, and I proceed to make them briefly.

1. Different inquiries usually employ different instruments (e.g., calorimeters, centrifuges, stains) and different specialized techniques (e.g., classificatory rather than metrical descriptions, Cartesian rather than generalized coordinates). Accordingly, if "method" is identified with the use of such apparatus and the exercise of such special skills, there are almost as many distinct scientific methods as there are distinct problems and departmentalized subject matters. I do not think that in the context of our present discussion, however, it is useful to make such an identification, or profitable to enumerate and examine the various methods conceived in this fashion.

2. It is a familiar fact that a broad domain of inquiry can be investigated from several points of view, in which alternative theoretical assumptions and modes of intellectual analysis are operative. To cite a trite example, thermal phenomena can be studied in terms of so-called pure thermodynamics, or they can be analyzed on the basis of ideas distinctive of statistical mechanics. Such differences in approach are frequently described as differences in method; and it is again apparent that if "method" is used in this sense, there certainly is a great plurality of distinctive scientific methods. Although such use of the word is legitimate, and in some contexts even clarifying, I prefer not to employ it in that fashion in this discussion, in part because the discussion of different scientific meth-

ods so conceived does not seem to me especially relevant to our purpose.

3. On the supposition that there is a fixed order in which investigations proceed and scientists make their discoveries, a number of influential scientists and philosophers have understood by the study of method the formulation of rules which adequately describe the postulated uniform behavior of scientists when engaged in their proper business, and which also specify the allegedly fixed series of steps that must be taken if success in inquiry is to be achieved. This conception of the nature and function of methodological study has been widely prevalent at least since the days of Francis Bacon, and current interest in the sociology of knowledge is in part dominated by it. Although the anthropology of science is a fascinating study, it fails to reveal that uniformity in scientific behavior which is tacitly assumed by those whose interest in scientific method is controlled by the desire to construct a logic and psychology of invention. Moreover, and this is perhaps the essential point, no accepta-



ble rules have yet been formulated which can serve as sure guides to scientific discovery. Accordingly, if "method" is understood in this sense, there is apparently no such thing as scientific method.

4. Finally, the word "method" can be understood to signify the way (or ways) in which statements in the empirical sciences are *evaluated* in the light of the available evidence for them, and the study of principles (whether explicit or unacknowledged) that underlie the acceptance or rejection of proposed conclusions of inquiry. Scientific method in this sense is the subject matter of the *logic* of inquiry; and its study has for its aim the articulation and analysis of criteria and standards involved in validating the outcome of investigations as warranted. I believe that there is such a body of principles (though their formulation is frequently vague and admittedly controversial), and that such principles are operative in the most primitive types of investigations conducted for the sake of narrowly practical ends, as well as in the most specialized and abstract branches of modern science. I also believe that in spite of the diversity of subject matters that may be investigated, the plurality of special skills that may be employed in them, or the variety of alternative conceptual schemes that may be used in exploring them, there is a connected and common body of general criteria which are actually employed in validating claims to authentic knowledge. At any rate, it is in this sense that I propose to use "scientific method" in the present context; and it is in this sense of the expression that I subscribe to the thesis that there is but one scientific method rather than a plurality of them.

II

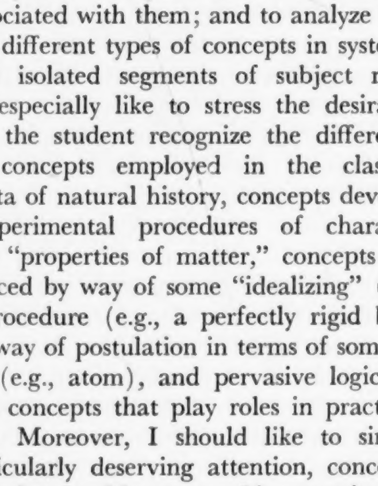
I must, however, supplement this credo with necessary qualifications. Though I think it is possible to distinguish between a proposition of method and a proposition with definite subject matter content, I do not believe that scientific method is a formal discipline which, like deductive logic, can be explored profitably without detailed consideration of the content of scientific knowledge or of the various operations that distinguish controlled inquiry. In any event, it seems to me that the canons which operate in scientific investigations cannot be properly appreciated except when exhibited in the context of concrete scientific procedure. Accounts of scientific method, like expositions of the history of science, mean little or nothing to those who are without some systematic training in concrete scientific problems. Accordingly, the elements of scientific method require to be presented in connection with the special materials of scientific knowledge, for only in this way does the latter become illumined by considerations of the former.

I therefore turn to a summary outline of what I take to be the main themes in the methodological study of science. I have found the following schematism useful in my own reflections. At the same time, I would not like to be understood as holding that the several items I shall mention fall into watertight compartments, or that the questions which arise under one heading can be treated independently of those which appear under another.

1. It is customary to say that science begins with a determination and discrimination of the "facts" relevant to its various problems, and I do not wish to quarrel with this characterization. Nevertheless, it is important to acquaint the student of general science with the complex character of fact-finding, and to call his attention to the considerable body

of assumptions that enter into any determination of what the facts actually are. For facts do not literally announce themselves to the inquirer, and the sheer immediate experience of qualitative situations does not constitute responsible factual study. On the contrary, immediate experience merely sets the problems for knowledge, so that it is one of the central tasks of science to interpret and suitably characterize the content of what is directly presented. Clearly, this involves the use of various special concepts. And indeed, for the layman, one of the most arresting features of modern science is the "distance," figuratively speaking, which separates the familiar distinctions of so-called common sense and the concepts employed in the pursuit and formulation of scientific knowledge.

Accordingly, an important aspect of scientific method is the study of concept formation, of the various ways in which the distinctions associated with the language of science are defined or introduced. I think it is illuminating in this connection to examine the extent to which experimental observation controls the introduction of new concepts, or determines the special way in which a concept is defined; to note the varying degrees of generality and "experimental remoteness" that are associated with them; and to analyze the function of different types of concepts in systematizing initially isolated segments of subject matter. I would especially like to stress the desirability of having the student recognize the differences between concepts employed in the classificatory schemata of natural history, concepts developed in the experimental procedures of characterizing various "properties of matter," concepts that are introduced by way of some "idealizing" (or limiting) procedure (e.g., a perfectly rigid body) or by the way of postulation in terms of some general theory (e.g., atom), and pervasive logico-mathematical concepts that play roles in practically all inquiry. Moreover, I should like to single out, as particularly deserving attention, concepts that are associated with measurable magnitudes, and in that context exhibit the need for understanding the principles underlying different scales of measurement.

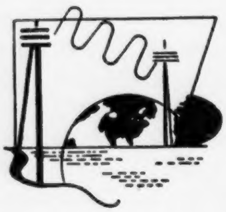


2. A second and closely related aspect of scientific method is the consideration of the structure and function of various kinds of statements that occur in the pursuit and formulation of scientific knowledge. It is an acknowledged task of the natural sciences to establish general propositions concerning the relations between definite features of things. But such propositions do not all have the same connections with experiment and observation, they are not interrelated in the same way, and their functions in inquiry and practice are also different. There are, moreover, propositions of other than general (or universal) form which are of concern to a working science. At one extreme are statements that describe some local conditions in a specific situation; at another pole there are comprehensive conceptual frameworks and general theories that contain no reference to determinate spatiotemporal regions; and intermediate between these two groups are propositions of varying degrees of generality, some of which may assert readily ascertainable relations between gross objects of common experience, whereas others may state functional dependencies between variable magnitudes that frequently involve subtle and indirect procedures of measurement. Again, some propositions function primarily as generalized leading or heuristic principles in wide ranges of inquiry; others are employed as means for "explaining" and systematically relating the connections asserted in other statements; still others are used for specifying the initial or boundary conditions in applying general statements to individual situations. The recognition of such distinctions contributes to the appreciation of the function of conjectural hypotheses in guiding and delimiting inquiry; to the understanding of the role of formal deductions in developing the meaning and consequences of initial assumptions; to the clarification of the nature of scientific explanations; and to exhibiting the value of systematically integrating propositions about quite heterogeneous materials into a coherent conceptual framework, whether for obtaining essential guides in emending such propositions when emendations are needed, or for securing a broader evidential basis for them than is possible when a proposition is taken in isolation from the others.

3. The remaining aspect of scientific method I must mention—and it is the central one for the sense of "method" here under discussion—is the examination of various criteria employed in evaluating the adequacy of proposed solutions to problems of inquiry. This is undoubtedly the most dif-

ficult and most unsettled part of the subject. Nevertheless, it seems to me that even here something useful can be said without undue dogmatism.

In the first place, the history of science makes amply evident that data of observation do not in general uniquely determine the form in which so-called laws of nature may be stated, nor the schema of explanation that may be provided for them. It seems clear, therefore, that principles of selection and rejection must be operative, which in favorable cases are at least practically decisive. In this connection the student should come to realize not only the suggestive value of observation and experiment for instituting new functional dependencies, but also their function in eliminating tentatively assumed connections. Again, the precisions with which experimental data are in agreement with assumptions made, as well as the congruence between theory and observation when undertaken by *different* inquirers and in a large *variety* of heterogeneous domains, are other objective factors that control the acceptance of proposed solutions to scientific problems; and I have no doubt that even elementary considerations on the nature of reliable sampling and statistical procedures help to clarify these matters. Moreover, as already indicated, an important function of comprehensive theories in science consists in their making it possible to use evidence that initially appears to confirm isolated propositions, as additional support for quite different propositions that are systematically related to the first. Indeed, in science as in daily life, the best accredited beliefs are not those which hang from but a single thread of evidence; they are those for which cumulative lines of argument are available. And methodological study can contribute to a more adequate understanding of the nature of science by showing how more responsibly held beliefs can be secured through the achievement of systematic coherence.



But if the possibility of error and sterile modes of attacking problems did not constantly attend the efforts of inquirers there would be no need for canons of evidence; and the history of science does indeed record an impressive quota of abandoned theories, corrected assumptions, and downright mistakes. Consequently, no account of scientific method is adequate which does not take notice of the ways inquiry can go astray, and which fails to recognize that factors other than cognitive ones may control the acceptance or rejection of propositions. In addition to more obvious sources of error, there are those which stem from a premature delimination of the set variables upon which the occurrence of a phenomenon may depend; there are the errors which come from the subtle influence of intellectual fashions and tacitly accepted philosophies; there are mistakes which are introduced by the pressure of social and political institutions, or by the understandable desire of scientists to justify their specialized labors to a larger community of men in terms of some assumed practical corollaries of their work; and, not least, there are the errors and lags in scientific development which arise from the pervasive fact of intellectual inertia, from reluctance to abandon ideas known to be doubtful until more adequate alternatives have been discovered, or until new points of view have been assimilated to more deeply held beliefs.

III

I have used up so much of my space in outlining what I take to be the content and value of scientific method that I can barely fit in some comments on the further question proposed in the formulation of our theme: Can scientific method be taught? I must, however, first confess my conviction that unless the student of science carries away with him from his technical studies a good sense of the methodological structures of science, those studies contribute little to his liberal education. Like many others, I have been dismayed by the slowly rising tide of cynical irrationalism that has engulfed so many influential writers, and not less dismayed because of the peculiar views they appear to hold concerning the nature of scientific reason—in spite of the fact that many of them have been exposed at one time or another to a respectable amount of instruction in the sciences. I have been especially discouraged by the fact that distinguished contributors to the sciences have frequently (though perhaps unwittingly) encouraged the debasement of the scientific enterprise—either through the irrelevant use to which they put current scientific doctrine in support of some questionable metaphysics, *Lebensphilosophie,* or political program, or through contrasting in disparaging but dubious manner the allegedly limited competence of scientific method with the satisfying fullness of some other form of human experience as an avenue to knowledge. I can only infer from such facts that it is possible to "do science" with reasonable proficiency, without possessing a ma-

ture and cultivated understanding of what one is doing and of how such doings are warranted. Nevertheless, we ought, I think, to aim at something more than technical proficiency as the fruit of science instruction. We certainly cannot afford to neglect technical proficiency; but we ought also to include as an essential part of our goal the development of that judiciously skeptical and yet tenaciously reasonable temper that characterizes at its best the continuum of inquiry that is science.

I am thus of the opinion that scientific method can be taught, and taught best not as a separate discipline or by precept, but in conjunction with the concrete materials of the sciences, and by example. The study of scientific method is a systematic reflection on the procedures of the sciences; and no greater pedagogic error can be committed than to give instruction in principles of method to students unfamiliar with scientific subject matter and practice. It is clear, however, that, if general courses in science are to be something else than training grounds for future specialists or preparations for parlor conversation, they must be so organized as to permit time for methodological reflection on the technical problems that are presented. There is a price that must be paid for such an organization, and in particular the conception that general courses in science should supply an encyclopedic compendium of useful knowledge must be abandoned. The essential point is that the materials to be included for study must be highly selected with a double end in view: to make the student competently familiar with some representative experimental and theoretical analyses of the natural sciences; and to provide him with clear examples of the operation of scientific method.

I feel confident that these are not incompatible principles of selection. Their adoption implicitly involves acceptance of the ideas underlying the "block-and-gap" schema which Professor Eric Rogers has so invitingly described—a schema which, I may add, we have been attempting for some time to institute at Columbia, although our efforts have thus far produced only meager results. In any event, it seems to me that an adequate general course in science must contain a good variety of materials drawn from a number of departmentalized disciplines; for no one example of scientific analysis can adequately illustrate the various phases of scientific method. Obviously, details of organization and pedagogic technique are not likely to be uniform at different institutions, since such details are inevitably controlled by local conditions involving student body, teaching staff, and physical equipment. What is essential, however, is that we keep our eyes on the goal of teaching science, not as a miscellaneous body of fixed doctrines, but as a series of intellectual achievements which are best understood as the products of a discriminating and flexible method of inquiry. Needless to say, complete success in such an enterprise is not easily achieved and may not be attainable. But, since even partial success is an important contribution to the development of a liberal intelligence, the prize is not unworthy of our best efforts.

The Nature of Protein Complexes

IRVING M. KLOTZ

Recipient in 1949 of the Eli Lilly Award in Biochemistry (ACS), Dr. Klotz (Ph.D., University of Chicago, 1940) has been associate professor of chemistry and biology at Northwestern University since 1947. In 1948 he also received the Army-Navy Certificate of Appreciation for wartime research.

COMBINATIONS between large protein molecules and small organic or inorganic molecules are the fundamental steps in a host of basic processes in living organisms. All the energy-producing chemical reactions, for example, proceed by complex-formation between specific proteins (enzymes) and energy-rich, small molecules. Basic physiological processes, such as muscular activity and the transmission of nerve impulses, depend on interactions of tissue proteins with small ions, particularly certain metallic ones. Similarly, the action of most drugs involves a combination of a small molecule with a protein endowed with a specific physiological function.

It has been realized for some time that an effective understanding of these interactions depends on an interpretation at the molecular level. With most biological responses, however, there are several steps between the experimental observation and the initial combination of the protein with the small molecule or ion, which make it very difficult to establish the properties of the complex in an unequivocal fashion. On the other hand, with a crystalline, homogeneous protein, one is dealing with a much simpler system in which associations with small molecules can be interpreted in a more straightforward fashion. It is for this reason that a great deal of effort has been devoted recently to the elucidation of the nature of these complexes of crystalline proteins.

In an early stage of investigation, a protein complex with a small molecule or ion might be represented in a very schematic manner by a diagram such as Figure 1. In this diagram each constituent of the complex is indicated, in an oversimplified way, as a sphere. For the small molecule, such an oversimplification may not be too drastic, but for the protein it may be very misleading. Unfortunately, however, very little is yet known about the structure of the protein framework. Nevertheless, for our present purpose, ignorance of the atomic arrangements in the framework is not a complete barrier to further progress, for we shall be interested largely in the nature of the site, indicated as a small protrusion in Figure 1, at which the small molecule is attached. Information on the molecular character of this site can be obtained without a knowledge of the internal arrangement within the protein molecule.

With this model we may proceed to formulate precisely several fundamental questions on the nature of the protein complex. First, we wish to know how many molecules of a given type can be held by the protein molecule under specified environmental conditions. Second, it is desirable to determine the strength of the bond between the protein and the small molecule. For most purposes, the strength of the bond is best expressed as the energy of combination, i.e., the energy which must be put into the complex to separate it into its two constituents. Finally, we should like to learn as much as possible about the configurational arrangement of the atoms which form the binding site on the protein, indicated grossly as a protrusion in Figure 1.

Complete answers to all these questions cannot be given as yet. Nevertheless, much information has been accumulated in the past few years, particularly in connection with complexes of the crystalline protein serum albumin. A description of the facts and theories with respect to this highly purified protein may be of assistance in the solution of related problems in more complex physiological situations.

The Number of Small Molecules Bound by the Protein

There are now approximately a dozen different methods of measurement of the extent to which a given molecule or ion is bound by a protein. Each of these can be classified into one of two categories: changes in the properties of the small molecule; changes in the properties of the protein. Ex-

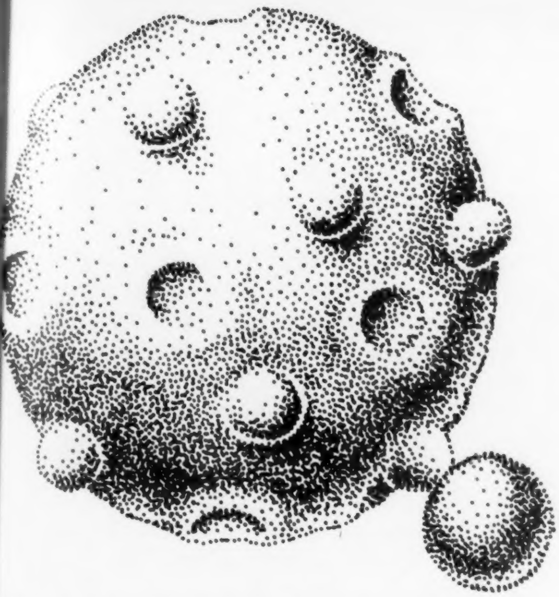


FIG. 1. Schematic diagram of complex between a large protein and a small molecule.

amples of changes in the properties of the small component are decreased ability to diffuse, or lowering in electrical conductivity. Changes in rate of sedimentation or in viscosity are illustrations of modifications in the properties of the protein.

It may be of interest to consider somewhat in detail one of the simplest, yet best, of these methods of measurement of binding—the dialysis-equilibrium method. The principle of the procedure can be understood readily from the diagram in Figure 2. A vessel is divided into two separate compartments by an especially prepared membrane which has submicroscopic holes in it sufficiently large that the small molecule, or ion, can pass from one side to the other, but small enough so that the protein molecule cannot do so. At the beginning of an experiment both compartments are filled with solutions containing only the small molecules. If the system is allowed to stand for a few hours, until equilibrium is obtained, an equal number of molecules will be found in each compartment (if the two volumes are equal). At this point we add protein molecules to one compartment, the right-hand one in Figure 2. Again we permit the system to come to equilibrium. Now we find that the total number of small molecules in the right-hand compartment is greater than in the left. Experimentally we can measure only the *total* number of small molecules in each chamber. However, it is a consequence of the fundamental laws of thermodynamics that the number of *free* small molecules in the left chamber must be equal to the number of free ones in the right. The difference between total small molecules on the right and total on the left thus gives the excess present on the right and hence the number bound by the large protein molecules.

The bound small molecules may be present in different proportions on different protein molecules of the same kind. For example, in Figure 2 one of the protein molecules has two small molecules attached, another has only one, and the third has none. For conciseness in representation of experimental data, it is customary to express the extent of binding as the *average* number of small molecules, or ions, per protein molecule. In the case illustrated in Figure 2, this average number bound would be one.

The average number of small molecules, or ions, bound per protein molecule increases with increasing concentration of the free, small molecule. As an example, some data on the number of copper ions bound by the protein serum albumin have been assembled in Table 1. At the very lowest Cu^{++} concentration, an average of only two ions is bound by each protein molecule. As the concentration is increased tenfold, the number of bound ions goes up also, but only threefold, to a value of 7. A further tenfold increase in concentration roughly doubles the number of bound ions, increasing the value to 13.

This trend in number of bound ions rather naturally leads to the question as to whether there is a limit in the number of ions that a single protein molecule can bind. That there must be a limit at some point is, of course, apparent since a protein molecule has only a finite surface and hence a limited amount of space to hold ions, or small molecules. Actually, however, if only very specialized sites on the surface are capable of binding specific ions or small molecules, we would expect the limit to be reached even before the entire surface is saturated.

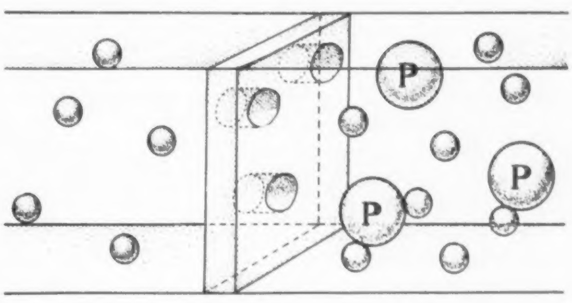


FIG. 2. Diagrammatic representation of the principle of the dialysis-equilibrium method for determining the extent of binding of a small molecule by a protein.

TABLE 1

INCREASE IN NUMBER OF COPPER IONS BOUND BY SERUM ALBUMIN WITH INCREASING COPPER CONCENTRATION

Concentration of Cu^{++} in Moles per Liter	Average Number of Cu^{++} Ions on Each Protein Molecule
0.0001	2
0.001	7
0.01	13

The experimental measurement of these limits for various small molecules is an exceedingly difficult problem. Nevertheless, it is apparent that even the approximate values that have been obtained so far (Table 2) are very much smaller than would be required to cover the entire surface. Thus for Cu^{++}, as an example, the maximum number of bound ions per albumin molecule has been found to be 16 (Table 2), yet there is adequate surface on the protein molecule to accommodate several hundred ions. Similar comparisons can be made for the other ions in Table 2. Thus, if at any time there are any doubts that the binding of ions and small molecules occurs only at special sites on a protein molecule, they may be quickly dissipated by citation of data such as those assembled in this table.

Strength of Bonds in Complexes

Turning to the question of energy of combination, we must keep in mind that more than one ion or small molecule may be bound by a single protein molecule and, hence, that the strengths of these bonds may or may not be the same. Certain theoretical considerations, in fact, would lead one to expect variations in bond strength with increasing number of attached molecules on a protein, even though all the attached molecules are identical. It is simpler, however, to approach this problem first from the experimental side.

Binding energies can be calculated from experimental data on the extent of binding at various concentrations of the small molecule. The calculations, however, are fairly involved and extensive and need not be discussed here. A typical series of results, nevertheless, can be presented concisely and are shown for copper-albumin complexes in Table 3.

TABLE 2

NUMBER OF SITES ON SERUM ALBUMIN MOLECULE AVAILABLE TO DIFFERENT IONS

Ion	Sites Available
Chloride	10
Dodecyl sulfate	14
Phenyl butyrate	24
o-Nitrophenolate	6
Methyl orange	22
Cupric	16

(Data taken from publications by Scatchard, Scheinberg and Armstrong, by Karush and Sonenberg, by Teresi and Luck, and by Klotz, Walker, Pivan, and Curme.)

It is of interest to note that the binding energy becomes smaller and smaller as more and more Cu^{++} ions are bound by the albumin. In other words, the strength of binding decreases as successive ions are added to the protein, despite the fact that each bound ion is identical with its predecessor and is on the same protein molecule. This trend in binding energies is characteristic of all complexes between proteins and small ions, or molecules. It is of interest, therefore, to examine the explanation for this trend, at the molecular level.

For this purpose, it is helpful to represent the energy of binding in terms of a well (Fig. 3), whose depth is proportional to the magnitude of the binding energy. Energetically speaking, the first complex between a protein and a small ion—e.g., P-Cu—is at the bottom of the left-hand well in Figure 3, whereas the dissociated components, in this example P and Cu^{++}, are at the ground level. Thus an input of energy is required to raise the complex from the bottom of the well to the surface, i.e., to dissociate a Cu^{++} ion from the complex. If the strength of binding of a second Cu^{++}, to give P-Cu_2 is less than that of the first, as the data in Table 3 indicate, then the depth of the well representing the second binding energy (center well of Fig. 3) should not be as great as that representing the first. The question we wish to raise is: What factors tend to make the second well less deep than the first; in other words, what factors cause a filling in of the well?

Careful analysis indicates that the well is filled in, in most cases, by two effects, the first of which is of electrostatic origin and the second of which may be attributed to certain probability considerations.

TABLE 3

VARIATION OF BOND STRENGTH FOR SUCCESSIVELY BOUND COPPER IONS ON SERUM ALBUMIN

Number of Ion Bound	Energy of Binding (Calories/Mole)
1	5,910
2	5,400
3	5,050
—	—
—	—
—	—
14	2,570
15	2,230
16	1,720

The electrostatic effect is simple to understand. When ions are bound, we are dealing with charged units and, furthermore, with species of like charge. It is to be expected, then, that the first bound Cu^{++} ion would exert an electrostatic repulsion toward an approaching second Cu^{++} ion. Even if the second Cu^{++} were bound, electrostatic repulsion between it and the first Cu^{++} would make it easier to dissociate the second bound ion. Thus the tendency of the second Cu^{++} to be bound by P-Cu is reduced, or in other words the strength of the bond is decreased. Diagrammatically speaking, the well (Fig. 3) would be filled in to some extent by electrostatic repulsion. Since this repulsion increases in magnitude as the number of bound Cu^{++} ions increases, the well becomes filled in more and more by the electrostatic effect as we proceed from left to right in Figure 3.

The probability factor may be appreciated by comparing the situation a Cu^{++} ion faces as it approaches P (Fig. 3) with that when it approaches P-Cu. In the second case the number of sites open to Cu^{++} is only 15, since 1 of the total of 16 is already occupied, whereas 16 were available to the first Cu^{++} approaching P. Thus the chances of a successful collision in the second case are only 15/16 that in the first, and hence the probability of forming the second complex, P-Cu_2, is less than that of forming P-Cu.

Another aspect of the probability factor, working against the existence of P-Cu_2 as compared to P-Cu, arises even after the complexes have been formed. With P-Cu_2, there are two positions from which a single Cu^{++} may be lost, since the first Cu^{++}, as well as the second, may come off, and in either case a P-Cu would be formed. Therefore, the chance of P-Cu_2 dissociating a single Cu^{++} is twice as much as that of P-Cu losing a single Cu^{++}. Again, we have a probability factor which tends to reduce the number of P-Cu_2 complexes. Energetically speaking, this corresponds to saying that it takes less energy to break up P-Cu_2 than to break up P-Cu. In terms of our schematic diagram

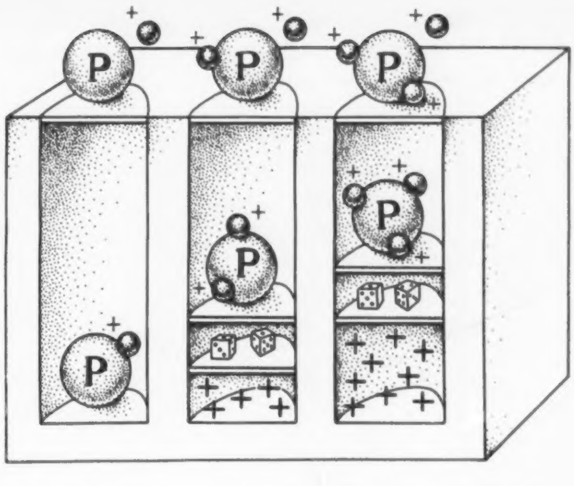


FIG. 3. Factors contributing to the decrease in binding energy of successively attached small ions.

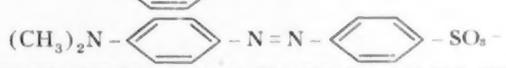
(Fig. 3), therefore, the second well must be filled in partially by the probability factor. In contrast to the electrostatic effect, however, the probability effect changes only slightly as we proceed to complexes with more and more Cu^{++} ions.

Binding energies may be compared for different types of small ions, as well as between different numbers of the same ion, on a specified protein. Some typical values of the energy of combination, for the formation of the first complex in each case, have been assembled in Table 4 for a series of negatively charged ions and the protein serum albumin. All values are in the neighborhood of 10 kilocalories or less and, thus, far smaller than the bond energies of strong covalent bonds, such as C-C or C-H, which are around 100 kilocalories.

It is also of interest to note from Table 4 that the energy of association with the protein increases with increasing molecular weight. Thus, the larger the molecule, the more likely it is to be bound by the albumin in the blood stream. Since bound molecules are, in general, inactive physiologically, these combinations with serum proteins have a profound effect on the biochemical action of mole-

TABLE 4

BINDING ENERGIES FOR COMPLEXES WITH SERUM ALBUMIN

Ion	Energy of Binding (Calories/Mole)
Cl^-	3,700
	4,865
	6,050
$CH_3CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2CH_2SO_4^-$	11,000

(Data taken from publications by Scatchard, Scheinberg and Armstrong, by Teresi and Luck, by Karush and Sonenberg, and by Klotz and Urquhart.)

cules introduced into the blood stream. Many drugs, for example, which are quite active against bacteria in test tube experiments are inactive in vivo because of combinations with serum proteins. Conversely, many substances which would produce drastic effects on certain tissues in vitro will not do so in the presence of plasma albumin. All these interactions must be appreciated before the physiological effects of various molecules can be predicted reliably.

Nature of Binding Site on Protein

In considering the characteristics of the locus of attachment on the surface of the protein, we must treat different classes of small molecules and ions separately. The present discussion, therefore, will be limited to complexes with negatively charged molecules, or anions, of the type listed in Table 4.

Although the suggestion had been made earlier, the first direct indication that anions may be attracted largely by positively charged sites on the protein came from the comparison of the degree of binding of pairs of compounds of practically identical structure, but with one carrying a negative charge. Thus, it was observed that the sulfanilate ion (upper molecule, Fig. 4) is bound much more strongly by serum albumin than the closely analogous compound sulfanilamide (lower molecule, Fig. 4), which does not carry a negative charge. It seems highly likely, therefore, that the protrusion (Fig. 4) which is attracting the sulfanilate ion is a positively charged structure.

At first thought, it seemed somewhat surprising that the presence of a negative charge on a small molecule should assist in the formation of a complex with a protein, for the proteins investigated themselves carry a net negative charge under physiological conditions, as well as in the laboratory experiments. It is necessary to keep in mind, however, that a net negative charge on a protein molecule merely implies an excess of negative over positive groups. Under the conditions studied,

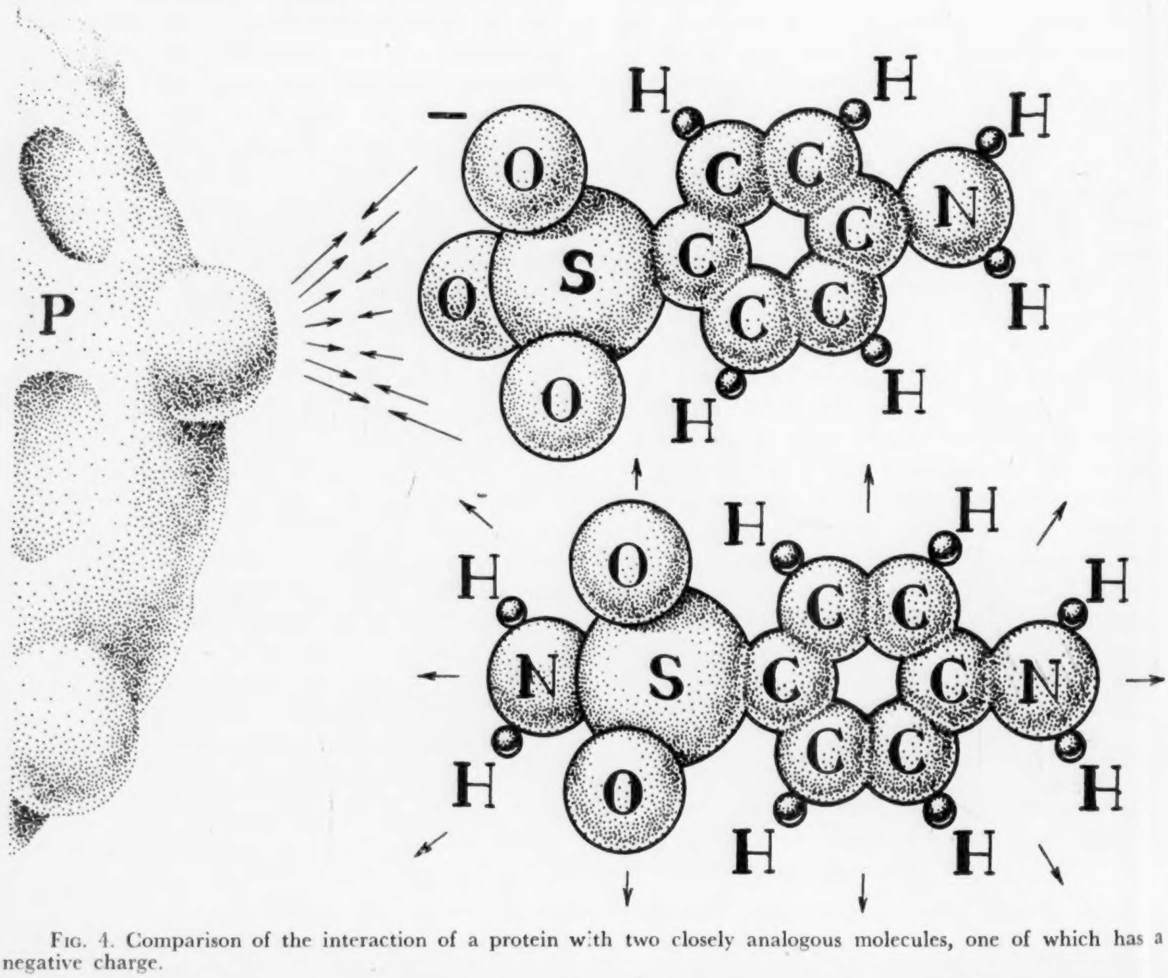


Fig. 4. Comparison of the interaction of a protein with two closely analogous molecules, one of which has a negative charge.

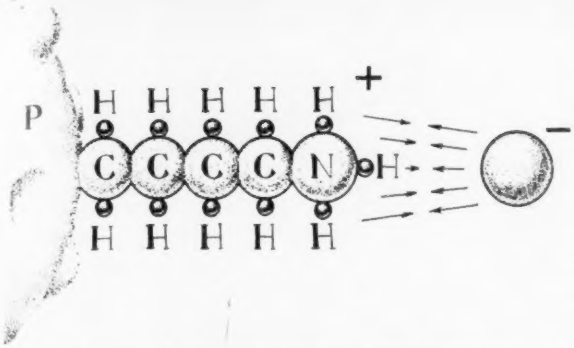


FIG. 5. Attraction of a small negative ion by the positively charged lysine side chain of a protein.

albumin has a substantial number of positively charged side chains which may protrude from the surface of the molecule. One of the three general types of positive side chain, the lysine residue, is illustrated in detail in Figure 5. It is groups of this kind which could attract negative ions.

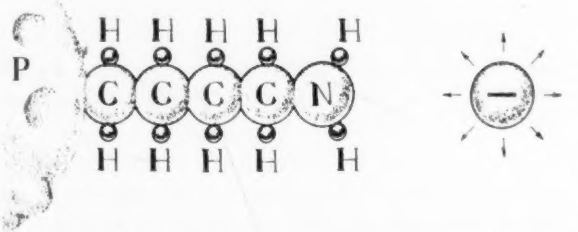


FIG. 6. Absence of any attraction of a small negative ion when the lysine side chain loses its positive charge in mildly alkaline solution.

Several lines of evidence substantiate the assumption that these positively charged side chains on the protein are the loci of attachment of small anions. Two methods of approach are worth considering in some detail.

It would be expected that any factor which removes the positive charge from these loci should reduce the extent of binding of anions markedly.

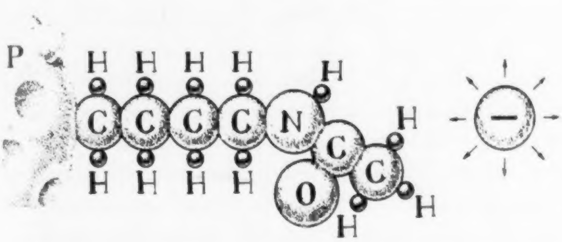


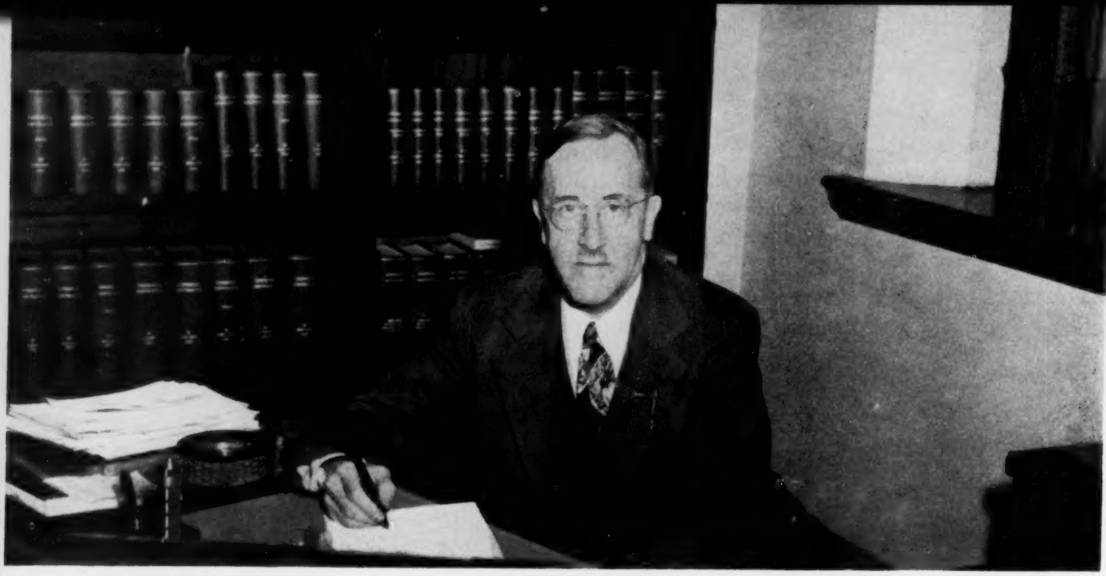
FIG. 7. Absence of any attraction of a small negative ion when the lysine side chain loses its positive charge because of the introduction of an acetyl group in place of two Hs.

One of the simplest methods of removing the positive charge from the side chains such as lysine is to increase the alkalinity of the solution. In mildly alkaline solutions one of the terminal H atoms shown in Figure 5 is lost and takes a positive charge with it. As a result the side chain assumes the structure shown in Figure 6. Without a positive charge, the side chain should no longer attract negative ions. Experiments show strikingly that albumin does not bind negative ions in mildly alkaline solution.

Since mild alkalinity sometimes produces irreversible changes in the framework of certain protein molecules, it has seemed desirable to corroborate the results described by experiments in neutral solution. In neutral solution, however, the lysine side chains on the protein in its natural state retain their positive charges. This apparent incompatibility in desired conditions can be circumvented, nevertheless, as a result of researches in the past few years which have led to methods of changing side chains chemically without injuring the framework of the protein molecule. One of these methods allows us to remove two H atoms from the terminal N (Fig. 5) and to replace them by a group of atoms called the "acetyl radical" (Fig. 7). One consequence of this chemical transformation is the loss of a positive charge by the lysine side chain. Again, then, we would expect the protein with the modified side chain (Fig. 7) to lose its ability to bind negative ions. The necessary experiments, which can now be carried out in neutral solution, show that binding properties are indeed lost. Thus we have confirmatory evidence that negatively charged small ions are held at positively charged loci on the surface of the protein molecule.

The foregoing discussion represents a rather cursory survey of some of the characteristics of representative complexes between proteins and small molecules, or ions. From this survey it is perhaps apparent that a great deal of progress has been made in filling details into the bare schematic representation shown in Figure 1.

Primary attention has been paid to complexes of well-defined, purified proteins such as crystalline serum albumin. It is, of course, a long way from the study of ion-protein complexes in relatively simple, artificial systems to the prediction of the effects of ions, or small molecules, under physiological conditions. Nevertheless, the elucidation of the nature of such interactions under artificial conditions cannot help but serve as a guide to the behavior of these molecules in living systems.



Roger Adams

President of the AAAS for 1950

WALTER J. MURPHY

Editor, Industrial and Engineering Chemistry, Analytical Chemistry, *and* Chemical and Engineering News, *and Director, American Chemical Society News Service, Washington, D. C.*

ROGER ADAMS, chairman of the Board of Directors of the American Chemical Society and head of the Department of Chemistry, University of Illinois, becomes president of the American Association for the Advancement of Science and its more than 43,000 members in 1950. Dr. Adams has occupied so many prominent positions in the field of chemistry and is so well known to the membership of these organizations, as well as to the scientific world in general, that a mere recitation of his accomplishments would fill many more pages than have been placed at our disposal.

The Adams family of Massachusetts, the same line that produced John and John Quincy Adams, had contributed much to the political, literary, and social development of the United States before Roger Adams made his advent at Boston, Massachusetts, on January 2, 1889, the youngest of four children. He received his early education in the public schools of Boston and Cambridge and entered Harvard University at sixteen; his undergraduate and graduate degrees followed in rapid succession: A.B., 1909; A.M., 1910; and Ph.D., 1912. He is also the recipient of six honorary Doctor of Science degrees—from Brooklyn Polytechnic Institute, Northwestern University, University of Rochester, Harvard University, University of Pennsylvania, and Yale. Such men as Professors Torrey and T. W. Richards, with whom he did his graduate work, did much to stimulate and direct his scientific interest in chemistry.

A traveling fellowship during 1912 and 1913 made it possible for him to continue his formal education in Germany. There he studied under Diels at the University of Berlin and under Willstaetter at the Kaiser Wilhelm Institute of Dahlem. This was his second trip abroad, the first having been to England in true undergraduate fashion, via cattle boat while he was a sophomore in college. He returned to this country and his alma mater in 1913, and for a year was research assistant to Professor C. L. Jackson and instructor in organic chemistry at Harvard. He continued there as instructor for two more years. His long connection with the University of Illinois began in September 1916, and for the past thirty-three years he has served that institution and the science of organic chemistry with all his energy and ability: as assistant professor of organic chemistry from 1916 to 1919, full professor from 1919 to 1926, and as head of the Department of Chemistry since 1926.

The researches he has conducted and directed in his laboratories at the University of Illinois have resulted in many important contributions to the science of organic chemistry. His enthusiasm is infectious, and his sound scientific training and experience have been a never-failing source of inspiration to graduate students, research associates, and colleagues alike. Some of the important investigations that have added to his distinction, that of his associates, and his department at the University of Illinois are: synthetic local anesthetics, including the commercial product "Butyn;" investigations of anthraquinones; structure of chaulmoogric and hydrocarpic acids (both from chaulmoogra oil) and the synthesis of acids with similar biological action; development of an improved catalyst (platinum oxide) and its use in catalytic reductions; restricted rotation in substituted biphenyls, cinnamic acids, and aromatic amines; elucidation of the structure of gossypol (the toxic principle in cottonseed), of tetrahydrocannabinol (the active principle of hemp known as marihuana or hashish and the synthesis of even more potent physiologically active compounds); and the senecio alkaloids.

Dr. Adams has long been active in the affairs of the American Chemical Society. He served as its president in 1935, director 1931–35 and 1940–49, associate editor of the *Journal of the American Chemical Society* 1922–32, and chairman of the Board of Directors since 1944. In the time he has held this office the membership of the Society has increased from 39,438 to 62,200.

His interest in furthering the professional and economic status of chemists and chemical engineers is well brought out in an article that appeared on page 1,706 of *Chemical and Engineering News* for October 10, 1945. The culmination of a great deal of thought on this subject by Dr. Adams, it shows that he recognizes the relationship between employer and professional employee is not a one-way street. It is no exaggeration to say that the pattern he evolved in this article for "what constitutes satisfactory employment conditions conducive to the contentment, satisfaction, and professional success of professional employees" has served as a model for many companies in the formulation of over-all professional employee-employer relations.

Dr. Adams' service to his country during both world wars materially enhanced his professional reputation. While serving as a major in the Chemical Warfare Service during World War I, he discovered diphenylamine-chloroarsine, better known as Adamsite—to mention but one of his scientific accomplishments in this period—which added luster to the best traditions of the Adams family in service to the United States.

When President Roosevelt set up the Office of Scientific Research and Development in 1941, Dr. Adams was appointed a member of the National Defense Research Committee and continued on it until 1947; his specific duty was to supervise the researches undertaken in chemistry and chemical engineering. He was one of those who passed on the general policy of the NDRC and on the budgets approved for investigations in various scientific fields. In his area were included the work on explosives and related materials, new gases for gas warfare, methods for determining their toxicity, the creation of materials to counteract wounds received from gas, methods for production of improved impregnated clothing, etc. Other activities involved improvement of gas masks, devising screening smoke, incendiaries, and flame-throwers, building units for producing oxygen in the field, and a miscellany of chemical engineering programs. This work won for him the Medal for Merit, the highest award given civilians by the U. S. government. The British government appointed him Honorary Commander of the Civilian Division of the Most Excellent Order of the British Empire.

Dr. Adams is receptive, often beyond reason, to requests that he undertake extra-curricular assignments, and he does not regard them as matters to be sloughed off carelessly. Instead, he calls on his seemingly inexhaustible energy and does an outstanding job.

In 1946 he went to Berlin as a scientific adviser to Lieutenant General Lucius D. Clay, at that time Deputy Military Governor of Germany. The work assigned to Adams was so diversified that he should have been twins or even triplets. He was sent to serve on the technical staff of the liquidation of the war potential committee, which formulated the research control program for postwar Germany; to learn about the possibility of resuming publication of scientific literature, especially in the field of chemistry; to look into the general situation of scientists; and to answer technical questions that arose in the Deputy Military Governor's Office.

In 1947 Dr. Adams headed a six-man scientific committee to advise the Supreme Commander of the Allied Forces in the Pacific on reorganization of Japanese science. The commission made an extensive tour of the islands, visited some twenty-five industrial plants, laboratories, and many universities, and conferred with the leading scientists in Japan. In 1948 he returned to Japan with five

other scientists to study scientific developments. When given an assignment, he invariably sees more possibilities in it than the immediate subject at hand. This was evidenced during his tour of duty with General Clay in Germany and again during his trips to Japan as adviser to General MacArthur, trips from which he returned deeply informed on such subjects as cormorant fishing, pearl culture, and geography.

He was elected to membership in the National Academy of Sciences in 1929. Membership at that time was limited to 250; it is now restricted to 450. He served on its Council from 1931 to 1937, and as Chemical Section chairman from 1938 to 1941. He was a member of the Executive Committee of the American Association for the Advancement of Science from 1941 to 1946, and again since 1948. He is an honorary Fellow of the Chemical Society of London and the Société Chimique de France. He is also a Fellow of the American Academy of Arts and Sciences, elected in 1928, and of the American Institute of Chemists, and honorary member of the Polish Chemical Society, American Institute of Chemists, and Phi Lambda Upsilon, a member of the Society of Chemical Industry, Harvey Society, American Philosophical Society since 1935, and of Sigma Xi, Phi Beta Kappa, Phi Kappa Phi, Gamma Alpha, and Alpha Tau Omega.

He was a member of the National Research Council Fellowship Board from 1928 to 1940, Committee on Chemistry and Chemical Technology 1927–30, Executive Committee 1929–30, of the Insect and Rodent Control Committee of the Office of Scientific Research and Development and of the National Academy of Sciences, and delegate to represent the National Research Council in the Council of the International Union of Chemistry at the 15th Conference of the Union at Amsterdam in September 1949.

In 1934–35 he was a member of President Roosevelt's Science Advisory Board; a member of the Advisory Council of the National Institute of Health 1936–39; a member of the National Inventors Council, and chairman of a committee appointed by the Foreign Economic Administrator to write a report on how to handle German research in industry and in the universities after the war; and is director, National Farm Chemurgic Council.

He has received many other honors for his scientific attainments. The American Chemical Society bestowed upon him its William H. Nichols Medal of the New York Section in 1927, the Willard Gibbs Medal of the Chicago Section in 1936 "for outstanding and fundamental contributions to synthetic organic chemistry, and for conspicuous achievement as a teacher of chemistry," the Theodore William Richards Medal of the Northeastern Section in 1946, and in 1947 the Priestley Medal, the highest award in American chemistry. Other awards include the Elliott Cresson Gold Medal of the Franklin Institute in 1944 "for discovery of original research adding to the sum of human knowledge irrespective of commercial value" and the Davy Medal of the Royal Society of London in 1945.

He originated the annual publications (now twenty-nine in number) known as "Organic Synthesis" which describe precisely and accurately methods for the preparation of organic chemicals for use in research work. More recently he has been editor in chief of "Organic Reactions," a widely accepted series of books (now five in number) which provide the organic research man with detailed and critical knowledge of the reactions frequently used in his experimentation. Dr. Adams is also the author of 327 papers which have appeared in scientific publications.

His devotion to science has in no way lessened but rather stimulated his enthusiasm for life in the fullest sense. He dislikes fishing, would not take first rank as a golfer, but as a tennis player at one time held the Chemistry Department championship at the University of Illinois. He is, or was, an interested philatelist, and has a valuable collection of uncancelled blocks of American stamps meticulously assembled and beautifully mounted. During the summer (except when the war prevented) he spends several weeks each year at his summer home in Vermont. Although he utilizes some of this time in scientific writing, he gets relaxation from golf, sailing, and ordinary physical labor at the cottage. His interest in politics, economics, and all that takes place around him is so well known as to need no comment.

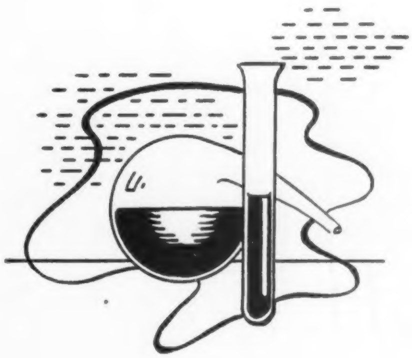
Everyone who is acquainted with Roger Adams knows his ability as a scientist. Probably the most characteristic thing about him is his real devotion to research. It seems absolutely impossible for him to divorce himself from it. Even during the world war, when he was working under the greatest tension, if he had a brief respite from the pressure of work at hand, invariably he would start to draw chemical formulas, explaining what the boys who were doing research for him back home were planning. His real fun is found in chemistry, and a new chemical reaction or new chemical compound never fails to evoke his keenest interest.

He is a hard worker but not a driver, and the men who work for him know that this love for science is entirely selfless, that any idea of personal glory or self-aggrandizement is furthest from his thought. Because of this he is an outstanding research leader and has amazing ability to keep things going at a high pitch. His zeal is contagious, and his students do their best to have something new to report to "The Chief," as he is affectionately known, every time he visits the laboratory when an important research problem is under way.

His loyalty is of the highest order, and he has always shown the deepest interest in the activities of his students. When his men leave the university he follows their careers, and if they return to visit their alma mater they always find his door wide open, unless some special conference is on, a welcome, and a willing and sympathetic ear. Also outstanding are his ease and lack of pretense in any company. He readily meets people at any level. In addition to being an outstanding scientist, he is perhaps equally outstanding as an administrator. He has a keen sense of humor, never permits himself to become confused, nor does he allow a meeting to get out of hand. He has the happy faculty of relieving the tension of a delicate situation by relating some humorous story or making some comment in facetious vein. He meets every problem with an open mind and lends an attentive ear to other people's suggestions. His memory is prodigious. Sometimes in discussing a subject he may write a note on a scrap of paper or the back of an old envelope and stuff it into his pocket. Those who know him best doubt that he ever finds these random notes, but the mere fact of writing them seems to impress the subject upon his mind.

In 1918, he was married to Lucile Wheeler, a graduate of Mount Holyoke, and formerly associate in home economics at the University of Illinois. Dr. and Mrs. Adams, together with their only daughter, Lucile, maintain a most attractive and hospitable home, in which students and faculty alike are always welcome, as well as their legion of friends.

He often participates in the extra-curricular activities of his students, sometimes with exemplary results. The story comes to me directly from his own beloved Noyes Laboratory that at one time he glanced through the laboratory door and was somewhat startled to see a poker game in full swing. Evincing no visible signs of disapproval, he sauntered over to the group of gradute students with a casual "Deal me in, boys," and in twenty minutes he had pocketed all the money. As the winner departed he admonished them, "Let this be a lesson to you. You can't make a living playing cards." His poker sessions with his alumni at various places throughout the country have often continued to teach them lessons! He never plays for high stakes, however; much as he delights in beating the boys who have trained under him—and he always plays to win, as in everything he does—nothing makes him more unhappy than to see someone lose who cannot afford to do so. Probably the truest and best evaluation of this man is that, in spite of all his honors, "Roger Adams is just a regular fellow." As one of his intimates says, "If I were shipwrecked on a distant island, I would hope that Roger Adams would be with me. I am sure that he would figure out some way of getting off and that we would have a whale of a good time while we were doing it."



Action Research Among American Indians*

LAURA THOMPSON

Well known for her work in the Fijis, Hawaii, Guam, Mexico, and among the Indians of the American Southwest, Dr. Thompson (Ph.D., California, 1933) has based her article on a paper read at the Twenty-ninth International Congress of Americanists which was in session in New York City last September.

THE Indian Personality and Administration Research was an interdisciplinary cooperative project, initiated and financed by the United States Office of Indian Affairs and directed by the University of Chicago's Committee on Human Development and the Society for Applied Anthropology. It was designed to illuminate the practical problem of how Indian Service policy and program might be improved in order to meet more effectively the needs of the Indians and to enhance their welfare. The project began in 1941 and was terminated in 1947. During the six years of its formal existence, eleven communities were investigated in five Indian tribes: Hopi, Navaho, Papago, Sioux, and Zuni. The major findings of the project are being presented in a number of tribal monographs, including *The Hopi Way* (Thompson and Joseph, 1944), *Warriors without Weapons* (Macgregor, 1946), *The Navaho* (Kluckhohn and Leighton, 1946), *Children of the People* (Leighton and Kluckhohn, 1947), *The Desert People* (Joseph, *et al.*, 1949); in numerous articles; and in a forthcoming terminal report entitled "Personality and Government" (Thompson: *America Indigena*, Jan. 1950, **10**, 1. See Chapter 2 for list of project publications).

As an integral part of the methodology developed in the course of the Indian Personality project, action research techniques were employed. The purpose of this article is to discuss briefly the action research program, with special reference to its effects on the participant-users of the research. Interest is focused not on the findings and recommendations of the project as a whole, which are, or will in due course be, available in published form, but rather on an attempt to evaluate the action research methodology itself as an instrument for the fundamental re-education of the volunteer user-participants.

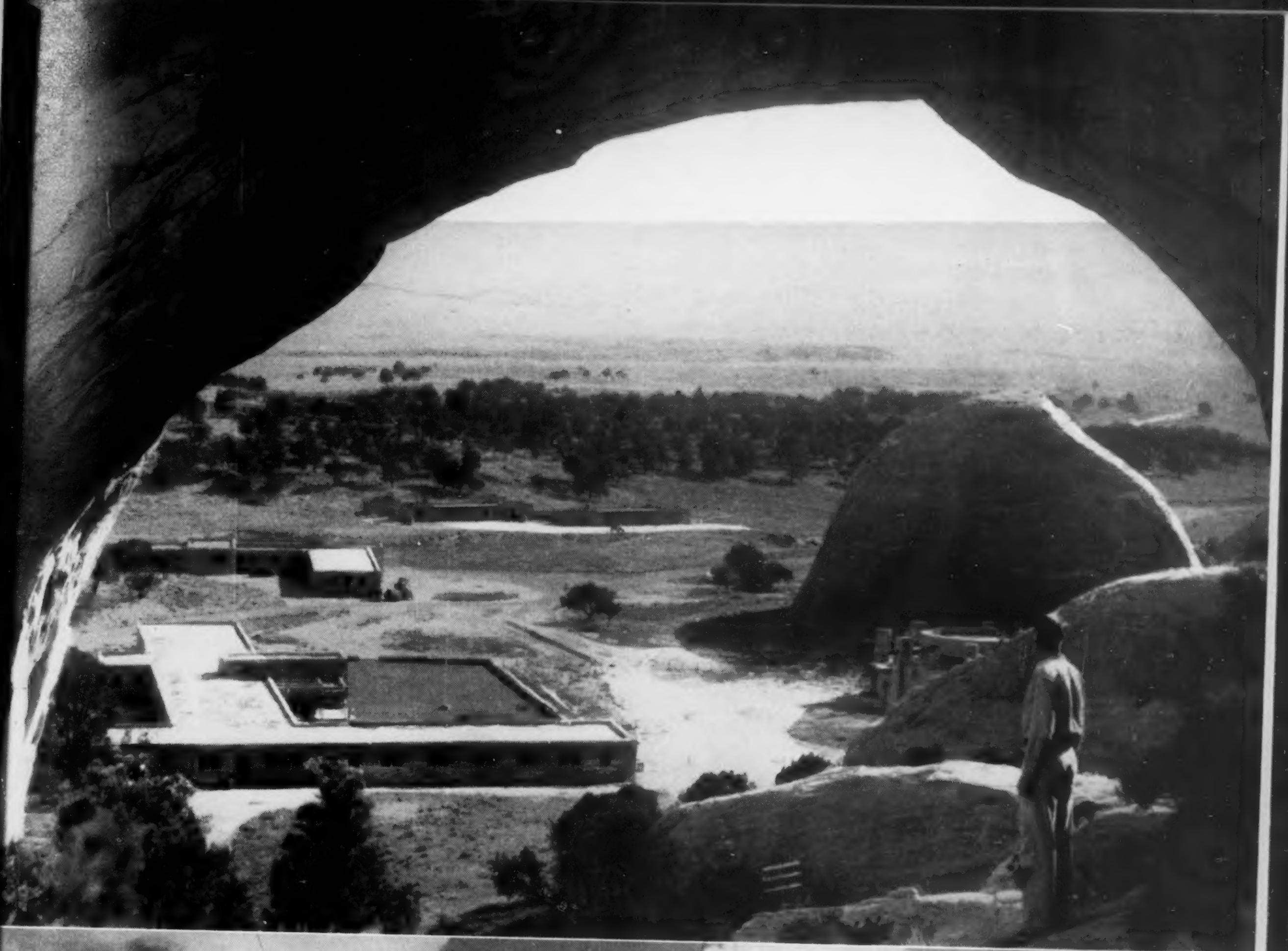
Action research is a relatively new type of cooperative social investigation whose development in this country before and during the second world war is closely associated with the names of Professor John Collier and the late Professor Kurt Lewin. A somewhat similar development in England is called operational research. Parallel developments are found elsewhere; for example, in the China Frontier Research Institute associated with the name of Professor Li An-che.

Action research is normally distinguished by the following characteristics: (1) it stems from an urgent practical problem, a felt need on the part of a group, and is generally solicited voluntarily by the potential users of the findings; (2) it involves both scientists and the user-volunteers as participants in a cooperative effort—namely, the solving of the practical problem; and (3) the scientists involved normally function both as scientist-technicians and as integrative or "democratic" leaders in Kurt Lewin's sense of the term. That is, they endeavor to stimulate, draw out, and foster the talents and leadership qualities of the members of the participant group and to minimize their own roles except as catalysts of group potentialities. In their role as integrative leaders, the staff scientists train, and supervise the work of, the volunteer user-participants.

This type of research has evolved from the acknowledged failure of more orthodox applied-sci-

* All photos from U. S. Indian Service.

Above: Looking down on Navaho Administration Building through Window Rock, Arizona, on the huge 16-million-acre reservation. *Below:* Sheep at the government's Experimental Sheep Laboratory, Navaho Indian Agency.



ence methods to solve urgent practical problems (such as that which motivated the Indian Personality Research) to the extent of bringing about a positive change in the "problematic situation" being investigated. For example, during the years after 1933, the Indian Service instigated interdisciplinary research, such as the Technical Cooperation Bureau of Indian Affairs projects, on many of the two hundred-odd Indian reservations in the United States. These projects were of the traditional survey type: that is, a team of scientists moved in; studied the problem for a few days or weeks, frequently antagonizing the local Indian residents; moved out again; and made a formal report to the authorities containing detailed recommendations for action. Here the responsibilities of the scientists ended. Frequently, owing to the impracticability of the recommendations, or to the difficulties inherent in their implementation, or to administrative inertia, the report was shelved. Thus little change in the local problematic situation resulted from the research, except perhaps an increase of antigovernment feeling on the part of the group that had been investigated. Also, social scientists involved in such research very often became critical of administrators who, they believed, failed to appreciate their work.

In an effort to overcome this situation, John Collier, then Indian Commissioner, urged the Indian Personality Research staff to experiment with action research techniques and to devise a methodology that would draw into the research endeavor local Indians and Indian Service personnel. Consequently, instead of being limited to professional scientists, the research staff was expanded to include Indian and non-Indian residents of the reservations being studied—teachers, nurses, physicians, range managers, conservationists, extension agents, etc., who volunteered to attend a fieldworkers' training seminar and to carry out field work for the project in their spare time.

The field problem of the Indian Personality Project was to study the development of Indian personality in the context of the social and natural environment in each of the eleven communities under investigation. The emotional, mental, and physical development of a random sample of children in each community, aged six to eighteen years, was investigated by means of participant observation, guided interviews, medical examinations, and a battery of relatively "culture-free" psychological tests. To carry out the plan of drawing lay users into active participation, an experimental twofold division of labor was worked out. On one hand, certain operations were performed by selected user-volunteers after special training and under technical guidance. These included administration of less complicated psychological tests, such as the Arthur Point Performance Scale, the Goodenough Draw-a-Man, Stewart's Emotional Response, and Bavelas' Moral Ideology tests, Piaget's Immanent Justice test, and a Free Drawing test; community interviewing regarding personality development patterns and life histories; and observation of the behavior of children in school, home, and community. On the other hand, operations which required highly specialized professional training for their execution were performed by staff scientists. These included administration of medical examinations, and of the Rorschach Psychodiagnostic and the Thematic Apperception tests; training and supervision of the volunteer fieldworkers; investigations to fill in gaps in the available literature regarding the local culture and geography; and integrative analysis and formulation of the field findings.

Before the field work began, a three weeks' training seminar was held at Santa Fe. Here one hundred fieldworkers, including both staff scientists and volunteer participants from the selected reservations, attended lectures and demonstrations by a faculty of leading scientists on psychology and personality development, social anthropology, ecology, public health, and the ethnology of the tribes being studied. Demonstrations of techniques to be used in the field work, such as Piaget's guided interview, were carried out behind a one-way screen. In all cases the fieldworkers were required to learn to use the various methods themselves by actual experimentation and practice. Teachers, for example, learned interviewing not only by listening to lectures on the subject, but by actually buttonholing people in the streets of Santa Fe, interviewing them, and submitting reports in written form to faculty members for criticism.

As might be expected, there was a good deal of opposition to the action research experiment, especially in the early stages of the project. Many teachers felt that they had been pressured into an undertaking which added just one more laborious task to an already heavy schedule, without the usual reward in the form of professional advancement. Some were shocked by their initial exposure to the principles of depth psychology and personality development which were brought home to them for the first time in the training seminar demonstrations. Others rejected the idea that they could learn anything useful to their own work from the so-called experts and their testing program. "With all your psychology," one teacher

A Navaho brother and sister visit neighbors near Navaho Agency Headquarters, Fort Defiance, Arizona. The girl's clothes and hairdress have been adapted from those of the Spanish and U. S. Army officers' wives of frontier days.

exclaimed, "you don't even know what I'm thinking!" Furthermore, many scientists predicted that the project findings would be "unscientific" and therefore worthless. Some Indian Service officials thought the research would be too highbrow to be understood by the agency personnel. Certain Indians considered it just one more field project which, like countless others in the past, would disturb and exploit the Indian for the benefit of white scientists.

When the field work started it was discovered that many of the teachers had never been in an Indian home. They had no idea of the living conditions, family backgrounds, and habits of the children they had been teaching for perhaps years. They had no contact with Indian community life and little understanding of Indian behavior, attitudes, and values. Their knowledge of their pupils was limited to superficial observations in the classroom and on the playground. The relation of school behavior to the child's total personality, and his developmental needs and trends in the context of home, community, and reservation, were not within their field of perception and awareness.

Naturally such teachers found it difficult and arduous to make their first home visits. They had been asked to do something quite new to them and outside what they considered their professional function. With the help of their seminar training, however, they ventured into their pupils' world as project participants. Because of their official position in the community, their questions about the children usually were not resented by the Indians, and, indeed, their personal interest was welcomed. But these first experiences in home visiting were likely to be full of surprises and even shocks. For example, on her first attempt at interviewing in an Indian village, one teacher entered a home where an infant was very ill. To answer the teacher's questions, the mother left the baby at one end of the room in charge of a relative. While the interview was in progress the child died. This news was whispered to the mother, who, nevertheless, politely continued to respond to the

teacher until the latter finally realized what had happened and terminated the interview.

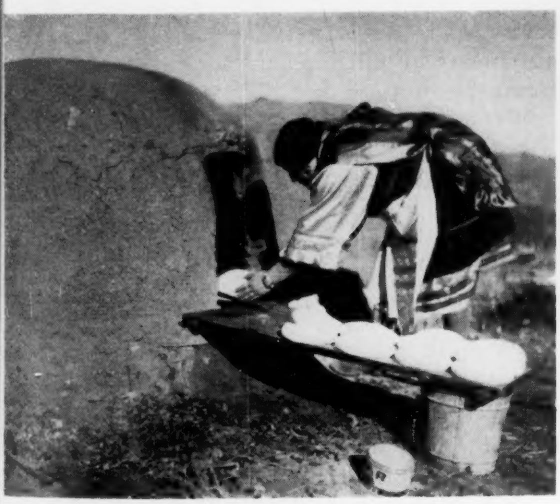
As the field work progressed, the teachers made many friends among both adults and children and became more at home in the Indian communities. Consequently they not only contributed essential data to the research project—data which would have taken many weeks or even months for a fieldworker from the outside to obtain—but they also learned a great deal of value to themselves. Knowledge of the changing part-Indian, part-Euro-American world of their pupils greatly aided them in their professional work and fostered their personal growth. As one participating teacher phrased it, "We learned to see the child not simply as a pupil in school, but as a developing personality in the total environment of home, school, and community."

Not only the home visiting but also the testing program contributed to the participant teachers' re-education. From an initial attitude of skepticism, indifference, or even hostility, many became interested and even enthusiastic as they learned from actual experience in psychological testing and also from the test findings, that these new techniques could contribute significantly to their understanding of their pupils and their problems. For instance, at the beginning of the testing program, an apparently shy Hopi boy of twelve, who did good work in school but played hooky much of the time, was in the second grade along with children much younger than himself. The test results revealed that this boy had high synthesizing, imaginative, and creative capacities, good powers of observation, originality, and a sense of humor. They indicated that he had not only a very well-balanced personality and a healthy body, but also a good social adjustment. Further investigation revealed that the boy came from a happy, harmonious, and conservative Hopi home, prominent in the pueblo ceremonial life, and that he not only was actively interested in ritual affairs but also helped his father in the fields, chopped wood for his mother, and herded his own flock of fifteen

An Indian farm on the Pine Ridge Indian Reservation in South Dakota. The Sioux of the Dakota Plains are the descendants of buffalo hunters.

A little Navaho girl sits on the family's sheepskin during one of its daily airings. Such sheepskin "mattresses" are in common use among the Navahos.

A Navaho silversmith from the wild wooded region southeast of Window Rock.



To this Zuni woman falls the responsibility of baking bread for several families in the out-of-door adobe oven. Except for her modern wrist watch and leather shoes, she is dressed in traditional pueblo style.

Part of Hotevilla Village at the Hopi Indian Agency in Arizona.

sheep. When the teachers learned, to their astonishment, that this boy was both highly gifted and well adjusted outside of school, they became interested in him and arranged special work in keeping with his needs and capacities. As a result of their attention, he advanced through several grades during the following year. This is but one case among many which helped to convince the teachers of the project's value to themselves and to the Indians. At the same time it deepened their appreciation of the capacities and potentialities of their pupils.

Most spectacular, perhaps, was the discovery through the testing program of the consistently high intelligence and balanced, sophisticated perception pattern characteristic of one of the tribes studied. The test results on the random sample of Hopi children indicated that this group had the highest I. Q. and the most balanced and, at the same time, complex and holistic mentality of any ethnic group (including the other Indian tribes studied and Euro-American groups) which had yet been investigated by means of relatively culture-free projective and performance tests. The findings on Hopi personality came as a complete surprise to all concerned and tended to increase the Indian Service staff's interest in, and respect toward, all the tribes, and also to reinforce the Indian's confidence in their own traditions and group powers in the face of strong, disintegrating pressures.

In sum, the experience of the volunteer participants in the project tended to deepen their awareness of, and respect for, societies and cultures different from their own, and to highlight the needs and problems of developing personalities in such societies within the limitations of the physical environment and in the context of the changing modern world situation.

Unfortunately, the Indian Personality project was terminated prematurely for political reasons before the action research program could be carried to completion. The plan was for staff scientists to return to the various reservations after the field findings had been analyzed and interpreted, in order to report the results of the research in detail to the volunteer participants and to confer with agency personnel on their most effective use for the improvement of Indian welfare. Despite the truncation of this aspect of the program through a change in bureaucratic policy, however, the findings indicate that the action research program operated, within limitations not inherent in the methodology but rather superimposed from without, as an effective instrument to re-educate many of the user-participants. The data indicate that significant changes occurred in the attitudes and values—indeed, in the culture—of the users, changes that were reflected in their habitual behavior; and they suggest the value of action research as an effective instrument for genuine group re-education.

The American Potash Industry

J. W. TURRENTINE

Dr. Turrentine (Ph.D., Cornell, 1908) came to Washington, D. C., in 1911 to participate in the potash researches then being organized by the U. S. Department of Agriculture. He continued his researches in the Department until 1935, when he was appointed president and chairman of the Board of Directors of the newly organized American Potash Institute, a position he held until the end of last year, at which time he retired with the title of president emeritus.

DOMESTIC independence with respect to essential raw materials has long been the aspiration of the research scientist, backed by American industrialists ever ready to enter upon new enterprises to improve the national economy. The development of the American potash industry within recent years affords a striking example of that generalization. It called for foresight and courage, foresight as to future agricultural needs and courage to face the competition of the powerful European potash cartel without knowing what forms that competition might assume.

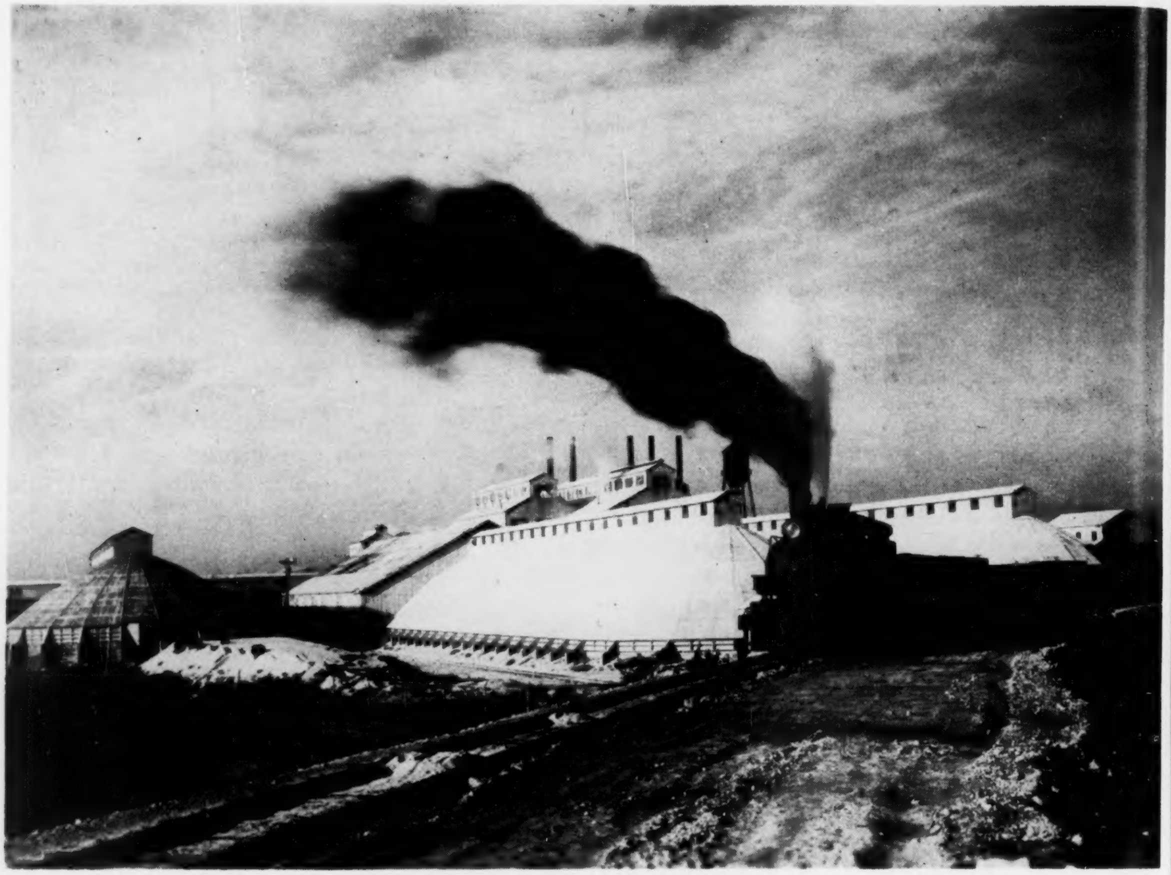
Great expansion in potash production by the American industry has taken place since 1938, the last normal prewar year. The logical place to start in tracing the development of the industry would seem to be that point in our agricultural history when we first realized our state of utter dependence on a single foreign source for our supplies of potash salts, which we had learned were essential in scientific crop nutrition. The date was 1910, and the single source of commercial potash salts was Germany. There the potash industry had been overexpanded to the point where surplus production and competitive selling were reducing to near bankruptcy all except the lowest-cost producers, with resulting chaos. To save the industry the German government organized a trust, closed down the less profitable mines, assigned production to the more profitable ones, and fixed the prices at which potash salts could be sold.

This policy resulted in the cancellation of favorable contracts with American buyers, who brought their troubles to Washington in the good old traditional manner and were told that the proper solution of the problem was the termination of dependence on Germany, through the establishment of domestic sources of potash—if such could be found and developed. In 1911 a Congressional appropriation became available to seek domestic occurrences of minerals, salines, brines, and seaweeds from which potash could be produced.

These explorations and surveys were most opportune, for, in 1914, with the outbreak of World War I, German importations abruptly ended and we were deprived of all potash supplies. Thereupon, under the impetus of a price increase from \$35 to \$500 per ton of 50 per cent muriate, practically all the potash-bearing raw materials (and industrial wastes) listed as the result of Federal surveys were placed under industrial development. By 1918, 128 production units were constructed, with an output of 209,000 tons of salts containing 54,800 tons K_2O and a rated but unrealized capacity considerably in excess of that.

The critical nature of the emergency did not permit technological research. On the contrary, potash was being extracted in many instances by main force and awkwardness. As a result, with the reappearance of German potash on the American market at a carefully regulated descending scale of prices, the wartime domestic industry faded away, with only three units surviving to recent years.

But potash research continued, and one enterprise that survived the postwar deflation in potash interest developed competitive processes and became a major factor in potash production—the American Potash and Chemical Corporation (the American Trona Corporation of World War I). Since that time, beginning with the extraction of potassium chloride from the complex brines of Searles Lake, California, by dint of continuous and persistent research, it has added product after product from the raw material processed, being currently credited with one of the outstanding chemical achievements of this country. Here phase-rule chemistry in its most intricate form was applied on a plant-wide scale and mechanized with the greatest precision. It was in this plant that the vacuum-cooling crystallization of potassium chloride was first applied on a large scale, yielding a product of 97 per cent purity and establishing



Potash storage warehouses, Carlsbad, New Mexico.

the now well-known "60 per cent muriate" as the standard potash grade.

Prior to 1926, the search for potash resources had been limited to what might be called surface aspects of the problem, outcroppings of potash minerals, the less pure strata of sodium chloride in salt mines already opened, and subterranean brines from salt springs and oil wells. No funds had been provided for the systematic exploration of the nation's great saline deposits, of which the most conspicuous and least explored was that of the vast Permian Basin underlying parts of Texas, New Mexico, and the states to the north and northeast. It was in this area, in Texas, that potash salts were found in solution in the natural brines from oil-well drillings. Then followed the discovery of fragments of crystalline potash minerals, indicating the presence of potash segregations in the saline strata penetrated by the borings.

On the basis of such evidence, meager at best, a bill was introduced in the Congress in 1924, "Authorizing Investigation by the United States Geological Survey to Determine Location and Extent of Potash Deposits in the United States," which, after much perseverance on the part of its proponents, and drastic amendments, including the designation of the U. S. Bureau of Mines as a participating agency, became a law in 1926.

Under this authorization, between 1926 and 1931, 24 core tests were drilled, 10 in Texas, 13 in New Mexico, and 1 in Utah. Beds of potash salts described as "of possible commercial interest" were encountered at depths of 373–2,737 feet, varying in thickness from 1 foot 6 inches to 8 feet 10 inches, and in potash content from 9.12 to 13.94 per cent K_2O.

The drilling procedure made use of the plunger type of drill through the overlying rock strata until the saline strata were encountered, whereupon the diamond core drill was substituted. With the use of saturated saline solutions as lubricants, complete cores of the saline strata were recovered, and their content of potash minerals identified and analyzed. This activity and the related publicity that preceded it alerted the oil-drillers exploring for oil in the Permian Basin to the possibility of

discovering potash deposits and taught the technique of identifying such deposits if encountered.

Accordingly, and concurrently, the Snowden and McSweeney Oil Company, exploring for oil in the neighborhood of Carlsbad in Eddy County, southeast New Mexico, discovered a potash deposit in the first core test for potash, beginning April 14, 1926. This deposit, at a depth of only 1,000 feet, proved of such richness and thickness as to leave no doubt as to its commercial value. Further exploration determined its lateral dimensions and proved that it rivals the best of the European deposits. Among the several strata of water-soluble potash minerals penetrated was a bed of sylvinite (a natural mixture of sylvite, potassium chloride, and halite, sodium chloride) containing 21 per cent K_2O, which was destined to become the major source of potash for American agriculture.

The United States Potash Company was organized by the Snowden and McSweeney Oil Company to exploit the Carlsbad deposits, and production began in 1931. The mining operation was highly mechanized, and the refinery was designed in accordance with the best technology of the day. Thus was first realized the dream of an American potash industry similar to that of Europe.

In the same field the Potash Company of America was organized in 1936, with a mine completely mechanized and a refinery built to apply the flotation process, the first industrial application of the familiar flotation principles to a water-soluble ore. This was followed in turn by the mine and refinery of the former Union Potash and Chemical Corporation, subsequently to be amalgamated with the International Minerals and Chemical Corporation, also with a mechanized mine and a refinery employing flotation methods in part. Including the American Potash and Chemical Corporation, these four companies are the major factors in the American potash industry.

Intermediate in scale of operations is the plant of Bonneville, Ltd., near Wendover, Utah, where the raw material is a brine found in the clay stratum underlying the salt crust that covers the Bonneville Flats or Salduro Marsh of the Salt Lake Basin. Here solar evaporation is employed to yield a mixture of crystalline potassium and sodium chlorides, subsequently separated by flotation.

While steadily increasing plant capacities, the major producers have added other chemicals to their list of products and thus have effected a diversification and full utilization of the constituents of their raw materials. Outstanding in this respect is the American Potash and Chemical Corporation with a list of products that includes potassium chloride of 98 per cent purity, designed for the fertilizer trade, and a product further refined for the chemical trade, as well as potassium sulfate, sodium sulfate, sodium carbonate, sodium borate, decahydrate, sodium metaborate, boric acid, bromine, potassium, sodium, and ammonium bromides, and lithium salts.

The potash ores of the Carlsbad area are too free from impurities to yield such an array of products; yet under production are potassium chloride of several degrees of purity and crystal size, 60 per cent muriate, 50 per cent muriate, and 22 per cent run-of-mine salts, potassium sulfate, sulfate of potash-magnesia, and potassium chlorate.

By 1935 the industry had attained production levels where it felt itself justified in initiating scientific research and educational activities. Accordingly, in that year the American Potash Institute was organized, with an experienced staff designed to direct agronomic, editorial, chemical and economic activities in the agricultural field and to provide consumer service in the scientific use of potash in crop production. To this end, supported by the American Potash and Chemical Corporation, the Potash Company of America, and the United States Potash Company, research fellowships are maintained in the leading agricultural research centers of the Continent, and through the medium of the agronomic journal, *Better Crops with Plant Food,* there is disseminated a large volume of diverse educational literature dealing with the many aspects of the profitable use of potash in agriculture.

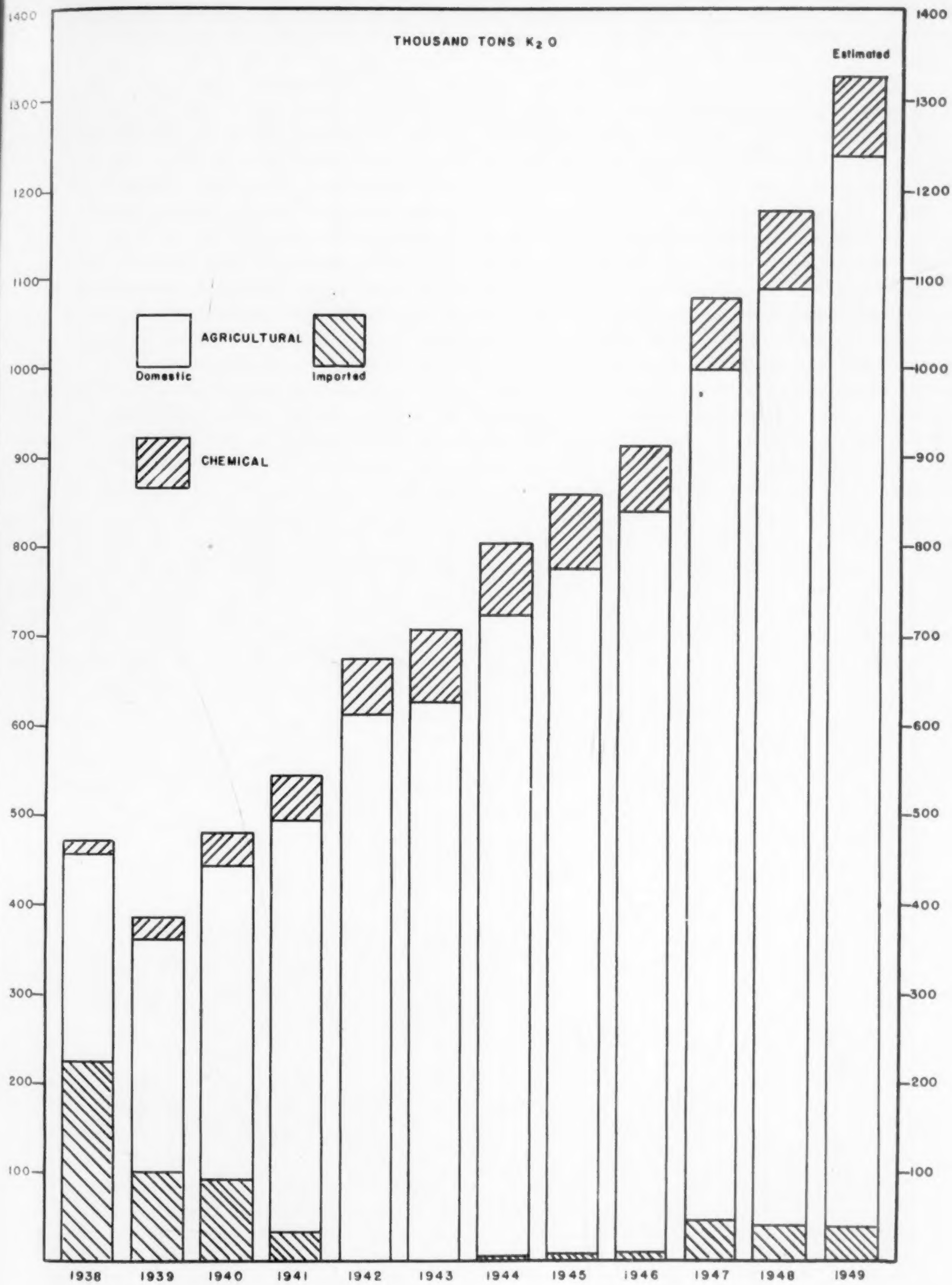
With these developments, the advent of World War II in 1939 found the nation in a radically different situation with respect to potash supplies than it was in 1914. On the later occasion the interested public greeted with considerable skepticism the announcement that the American potash industry was then prepared to take care of the nation's potash requirements, for it was known that, up to September of that year, we still had been importing a considerable percentage of these. What was not so generally known was that we had been exporting a substantial proportion of our production, which could and would be diverted immediately into the domestic market; that we had large expansions in productive capacity under way; that we had great reserves of unrefined run-of-mine salts readily available to equal any deficit in the refined salts that might develop; that potassium sulfate, formerly largely imported, could and promptly would, be produced in larger quantities.

Potash storage and equipment for loading.

After the war, the nation entered upon a reconstruction period of ever-increasing demand for agricultural products, calling for more and more potash wherewith to grow them. There was no let-up in the potash industry's efforts to meet the requirements. Thus, from an output of 535,000 tons of potash salts equivalent to 317,000 tons K_2O in 1938, production has increased each succeeding year, reaching a volume allowing a total of deliveries in 1948 of 2,132,512 tons of salts, equivalent to 1,133,773 tons K_2O (see chart). The steady increase was achieved under the many wartime handicaps that confronted production industries in general, but without the special Federal dispensations of capital and other aids so liberally given other industries whose products were regarded as more intimately tied in with the war effort. Superimposed was the task, voluntarily assumed by the producers, of supplying Canada, Puerto Rico, Hawaii, Cuba, and the good neighbors to the south of us. Even lend-lease came to us for its quota.

The chemical industries, which in 1938 consumed some 14,903 tons K_2O in their numerous manufactures, had increased their estimated requirements under the impetus of wartime demands to 100,000 tons K_2O by the war's end, dropping back to a peacetime requirement of 88,026 tons K_2O in 1948.

This record of performance was achieved without a price increase for the major grade, 60 per cent muriate, which makes up some 90 per cent of the total. In fact, prices have decreased. Prior to 1947 potash prices were quoted C.I.F. Atlantic and Gulf ports. Since that date, they have been quoted F.O.B. point of origin. Applied is a maximum seasonal discount of 12 per cent from the list price under which some 90 per cent of total sales are made. On the old C.I.F. basis, the per unit price of 1910–14 was 71.4 cents for muriate. By 1946 this price had been reduced to 47.1 cents (with the 12 per cent discount applied), a reduction of 24.3 cents per unit. The 1914 price of \$35 per ton for 50 per cent muriate is comparable to the 1946 price of \$28.26 per ton for 60 per cent



Potash deliveries—agricultural and chemical—North America.

muriate. The increase in concentration from 50 per cent to 60 per cent represents a corresponding decrease in transportation charges per unit K_2O. The current F.O.B. price under the 12 per cent discount is 33 cents per unit for the 60 per cent grade of muriate. As compared to the former C.I.F. price, this is a further reduction at many points of delivery.

Thus, on the basis of production and price records, the American potash industry has shown its entire competence to meet all the nation's more essential needs for potash salts for the agricultural and chemical industries, during not only the critical period of World War II but also subsequent years. In 1948 potash salts were shipped by the primary producers to 45 states and the District of Columbia. Georgia and Ohio practically tied for first place with deliveries of some 88,550 tons K_2O, followed in order by Illinois, Virginia, North Carolina, and Florida. State deliveries, however, cannot be taken as synonymous with state consumption, because 95 per cent of the potash salts are sold wholesale and in carlots to the fertilizer-mixing industry, which functions as the retail agency distributing the potash to the ultimate consumer, the farmer. From the larger mixing plants, the products are commonly shipped across state borders into neighboring and sometimes distant states. Under such circumstances, therefore, state consumption may differ widely from state deliveries.

The mixtures that comprise commercial fertilizers, as is well known, are carriers of the major crop nutrients, compounds of nitrogen, phosphorus, and potassium, to which may be added the minor but still essential nutrients, such as magnesium, boron, and others. These mixtures are compounded in various ratios as determined by local or regional crop requirements, the nutritive status of the soils as determined by soil tests, the availability of supplies, and, in too many situations, habit and tradition. Under this system the potash content may vary from 0 to 27 per cent K_2O.

Several factors are responsible for the expanding market. Foremost is the phenomenal increase in gross farm income, which reached the record total of 31 billion dollars in 1948. This may be contrasted with the gross farm income of 9.4 billion dollars in 1938. It is a matter of statistical record that the farmer's expenditures for fertilizers rise and fall with his income and in a close ratio thereto; he habitually spends for fertilizers so many cents out of each dollar of income, though the amount differs widely among agricultural areas—for example, 16 cents in the Southeast and 3 cents in the Midwest. With an income of 31 billion dollars, the farmer has ample cash to purchase plant food

Interior of potash mine, Carlsbad, New Mexico.

more nearly in the quantities and of the grades he has been taught to use by his advisers.

Thus education has become another important factor in the phenomenal increase in potash consumption—education based on research and field demonstration imparted to the farmer by many zealous Federal, state, and county agricultural agencies. Diagnostic techniques for determining the fertility status of soils and the nutritional status of the crops growing thereon have been adopted. Principal among these are the soil analyses made by state laboratories to which farmers can send their soil samples. These tests reveal the presence, or more frequently the absence, of potash in forms available for crop nutrition, thus providing authentic information for the farmer's guidance.

Related thereto is our growing knowledge of what constitutes balanced nutrition for the major crops. Crops are sampled by collecting leaves or other parts that can be analyzed for their plant-food content. This technique is resulting in the establishment of the so-called critical levels of potash content that are characteristic of the respective crops, and below which potash deficiency causes decreased crop yields.

Contributing also to the increase in potash consumption has been the changing pattern of American agriculture. Increasing interest in soil conservation and in the adoption of the various practices that enter into that fundamentally important program has been conspicuous in this respect. Closely related is the fertilized pasture, a revolutionary new development, particularly in the South, where the potentialities of a livestock industry are being so widely demonstrated as an important phase of diversification to relieve dependence on cotton and the one-crop system. For the fertilized pasture, legume-grass mixtures are prescribed, with liberal applications of high-potash fertilizer. Alfalfa is being emphasized in the South, where successful growth has now been made possible by adequate high-potash fertilization, provided borax is included. At present Ladino clover is competing with alfalfa in popularity.

Among the changing patterns, mention should be made of the radical new practices in the growing of the corn crop. It has been demonstrated that, with greatly increased fertilization applied to closely planted, adaptable hybrids, yields can be more than doubled. In this new development increased applications of nitrogen compounds are the major feature, although a balanced ratio of potash is likewise essential. With high wages for farm labor, yields per acre take on added importance in determining farm profits, and the adequate use of fertilizers assists materially in assuring a profit on every dollar expended. Such measures not only increase the farmer's income, but also enhance his economic stability, rendering him less vulnerable to adverse changes in the economic pattern and giving added stability to the industries dependent upon him as the ultimate consumer of their products.

With rapidly rising potash production, the magnitude of the nation's potash reserves and their life expectancy at the current rate of production are serious questions. A survey of reserves, financed by the four major producers, was made by Samuel H. Dolbear, whose findings were presented in *Potash Reserves of the United States,* issued by the American Potash Institute in 1945. This report states:

Known resources of potash in brines and in highly soluble salts of deposits now under production amount to 107 million tons of actual potash (K_2O) of which 73 million tons are estimated to be recoverable.

Possible reserves of sylvite yet undeveloped in the New Mexico field may add as much as 400 million tons to these reserves.

Polyhalite, a mineral containing soluble potash, has been encountered in the Permian Basin over an area of 40,000 square miles. Beds explored by drilling and underground work in the Carlsbad area of New Mexico contain huge proved reserves. Proved reserves are estimated at 140 million tons of K_2O and there is in addition over 100 million tons of K_2O in probable reserves, with possible reserves several times these figures. The total gross potash (K_2O) content of proved and probable polyhalite is therefore over 240 million tons in and adjacent to the present potash operations in the Carlsbad area. The degree of probability in this case is of such character that the proved and probable figures have been combined in estimating reserves.

Interest continues in the expansion of the American potash industry. Exploration by one new company in the Carlsbad area has prompted 47 deep drillings to determine the nature and extent of the potash deposits underlying its holdings. In addition, 22 leases and 177 permits have been issued by the Land Office of the U. S. Department of the Interior for further prospecting for potash on public lands in New Mexico, California, Utah, and Colorado. With an American potash industry now developed to an extent where the agricultural requirements of the North American continent are being easily met, we many anticipate that future demands of American agriculture, as it applies increasing quantities of plant foods in the scientific ratios to reduce the costs of crop production, can be handled from ample reserves efficiently utilized by private industry.

The Gorilla—Largest Living Primate

DAVID P. WILLOUGHBY

A native of New Orleans, Mr. Willoughby has lived in California since 1912 and has been on the staff of the California Institute of Technology as scientific illustrator in Vertebrate Paleontology since 1938. During recent years he has been engaged chiefly in research in the comparative anatomy (osteological) of mammals, living and extinct, principally the horse and its relatives.

THE discovery by paleontologists during recent years of the remains of a number of ancestral "ape men" of reputed gigantic size has brought renewed interest in man's origin, evolution, and relationships. The available remains (principally jaw fragments and teeth) of such specimens as the Asiatic ape men *Meganthropus* and *Gigantopithecus*,* and the South African Swartkrans man, show that certain long-extinct races of humans, or subhumans, were of much more robust and massive physique than any race of modern men. But the fact that some of the skeletal elements, notably the molar teeth, of these specimens are 50–80 per cent larger than in modern man is not sufficient basis for assuming (as was done in some of the first news accounts) that these remote ancestors of man were of a stature in direct proportion to the size of their teeth and jaws. Such proportioning would have made them 8–10 feet in height! As may be seen in the jaws and teeth of man's nearest living relative, the gorilla, the lower part of the face, in an anthropoid having "beastlike" proportions in this feature, may be pronouncedly heavy and massive without being accompanied by great stature. In view of the known high variability of the physical make-up in primates generally, conservative anthropologists recognize that a tooth, or even an entire jaw, is not an ample basis from which to judge accurately the bodily size of the individual to whom it belonged.

Nevertheless, it is possible that certain well-adapted races or species of early men were taller, as well as more heavily built, than modern man. If this be true, the evolutionary history of man's physique and stature may be a parallel to that of the horse. In the case of this familiar animal, abundant evidence shows that its earliest ancestors of fifty million years ago were of small stature (*Eohippus*, the "Dawn horse," was of about the size of a fox), and that succeeding generations grew increasingly larger up until the time of the last glacial retreat, some 15,000 or more years ago. Then, probably because of changed environment and reduced food supply, the horse gradually diminished in size from an animal standing 58 or 60 inches in shoulder height (some species were even as tall as modern draft horses) to a comparative runt of 48–50 inches. This latter wild horse was the foundation stock from which the present-day domesticated breeds—nearly all of them much larger in size—have been developed. Regression to smaller size is sometimes a sign of forthcoming extinction. Such regression presumably occurred in certain early lines of the horse genealogy. But that it is happening to man today is contradicted (despite the evidence of his giant ancestors) by the fact that he is now largely the master of his food and environment, and that he is increasing in stature steadily, generation by generation.

Among the living higher primates, however, man, despite his erect posture and tall stature, is still far inferior in general bulk to that giant ape, the gorilla. But just how big is a gorilla? Even this particular question has a history.

The gorilla, particularly the species known as the mountain gorilla, was a generation ago so reduced in numbers by hunters, both native and European, as to be approaching extinction. Today, thanks to the timely action of concerned officials in enacting laws preventing or limiting the

* Weidenreich (1946, p. 59) points out that this name, being that of a giant man, rather than a giant ape, should be *Gigantanthropus* rather than *Gigantopithecus*. Some other anthropologists, however, regard the teeth as being those of a giant orangutan or some other anthropoid.

further destruction of the wild gorilla population, these exceedingly interesting animals are on the increase. But, before protective restrictions were in force, and before the gorilla was the well-publicized animal to be seen in many a large zoo today, the hunting of these giant primates was conducted on lines similar to the hunting of any other imposing and desired museum specimen. In these early gorilla hunts, the object of the hunter naturally was to obtain the largest and finest individual possible. And, as the adult male gorilla is of much greater stature and bulk than the female, the biggest male in a troop was usually the specimen sought. As a result of such wild-killed "specimens" being measured and recorded, gradually a literature on the size of the gorilla, particularly of adult males thought to be of "record" dimensions, came into existence. From these early sportsmens' records, most popular works on natural history have drawn their information as to the size of the gorilla.

To state, as is commonly done, merely the size of a large or "record" specimen, however, is to provide only a vague idea of the *average* or *typical* size of the species in question; and it is always in relation to the typical (rather than the exceptional) specimen that comparisons should be made. Being interested, both as a zoological illustrator and as a student of natural history, in just what a typical-sized and a maximum-sized gorilla would measure, I have made an extensive study of the physical characteristics of anthropoid apes in general and of the gorilla in particular. The sources of the information for this study will be indicated in the following review.

First, there are several good reasons why so few statistics on the bodily size of the gorilla are available. For one thing, rarely is it possible, or is the trouble taken, to tame a specimen, even a small subadult, to the extent that the animal will submit to the taking of measurements of its body. Again, there is the fact that in the wild state gorillas are now largely protected by law, and the killing of them thereby largely abolished; hence, the comparatively limited data on the physical proportions of gorillas (and of apes in general) pertain mostly to embalmed specimens and skeletal material. A third source of information is the external dimensions of a fair number of legally wild-killed specimens of which field measurements were taken while the body still retained its living (unshrunken) proportions. Finally, in a very small number of immature or female gorillas, measurements have been obtained of the living animal.

Perhaps the first specific reference to the size of the gorilla was that reported by the English traveler and writer Thomas Henry Bowdich (1791–1824). He stated, "on the authority of the natives of the Gaboon," that this ape was generally about 5 feet in height. This probably referred to the height of the gorilla while in its normal, stooping posture. In 1859, the first embalmed gorilla specimen to reach England was described by the famous anatomist Richard Owen as being 5 feet 6 inches in standing height, 42 inches in sitting height, and with arms each 40 inches long.

The first person really to publicize the wild, living gorilla was the famous hunter Paul Du Chaillu, who in 1861 wrote a book, *Explorations and Adventures in Equatorial Africa,* in which he recounted his experiences with this and other jungle animals. The largest of nine gorillas, an adult male, shot by Du Chaillu, stood 5 feet 8 inches in height. Du Chaillu stated, however, that adult male gorillas ranged in height from 5 feet 2 inches to 6 feet 2 inches.

All the foregoing gorillas were of the coast species (*Gorilla gorilla*), found in the lowland jungles of French Equatorial Africa on the west coast. The mountain gorilla (*Gorilla beringei*), which lives in a relatively small area on the slopes of the volcanic mountains around Lake Kivu, in the extreme eastern part of the Belgian Congo, was "discovered" by a white man only as recently as 1902. Between the respective ranges of the coast and the mountain gorilla, there are more than 650 miles of dense, equatorial forest in which no living gorillas have ever been found.

One of the finest photographs ever taken of a wild-killed gorilla was that obtained by the German trader H. Paschen. It shows the ape sitting on the ground with natives alongside it (Fig. 1). The specimen, an adult male, was killed near Tonsu, in the Cameroons, in 1900. Its skin and skeleton were prepared by J. F. G. Umlauff, of Hamburg, Germany, and were exhibited in the Museum Umlauff of that city. Later, the stuffed specimen was purchased for 20,000 marks (then $4,760) by Lord Walter Rothschild, and placed in the Rothschild Museum at Tring, Hertfordshire, England. Particular interest attaches to this specimen in that it was stated to be of gigantic size. Its length, lying, was given as 2.07 meters (6 feet 9.5 inches) and its span (spread of arms) as 2.8 meters (9 feet 2.25 inches). Upon investigation, however, it was found that the "length, lying" was taken from the crown of the head to the end of the middle toe, not to the heel. Again, if the armspread of this gorilla was 9 feet 2.25 inches, its standing height would have

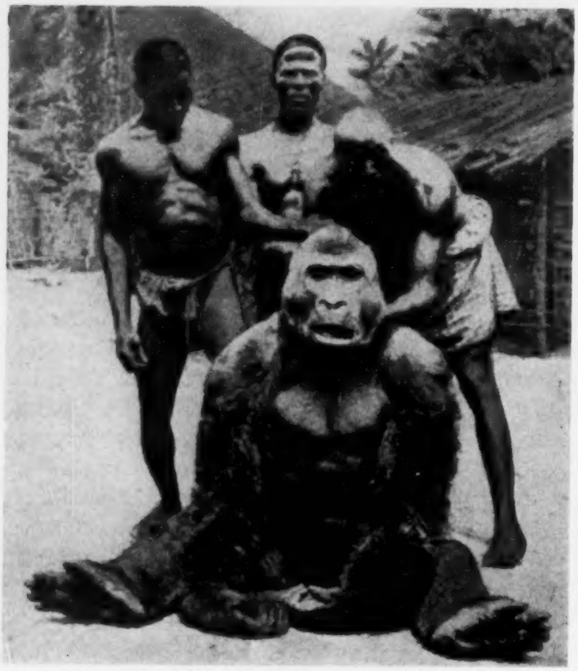


FIG. 1. Adult male gorilla shot by H. Paschen near Yaounde, Cameroons, in 1900. The mounted specimen is now in the Rothschild Museum, Tring, England. (*Photo by H. Paschen*).

been only about 6 feet 1.5 inches. But, to confuse matters, in *The American Natural History,* by W. T. Hornaday, the author says that this same specimen, shot by H. Paschen, stood 5 feet 6 inches in height and weighed (by estimation) 500 pounds. In *The Living Animals of the World,* edited by C. J. Cornish, the height is given as 5 feet 5 inches, span "over eight feet," and weight 400 pounds. In Brehm's *Tierleben* the height is given as "over 2 meters" (6 feet 7 inches) and weight, estimated, 250 kilograms (551 pounds).

Whatever the actual size of this oft-mentioned specimen, it is clear that its height could not have varied between 5 feet 5 inches and 6 feet 9.5 inches; hence, perhaps *all* the figures are wrong. A clue to the animal having been *under* 6 feet in height, however, is furnished by Lord Rothschild, who, after having made an extensive study of the physical characteristics of gorillas, stated: "Adult males of the three well-defined races vary in height from 5 to 6 feet, and there is no specimen preserved over 6 feet in height. . . ." This Cameroons specimen, being in Rothschild's possession at the time, presumably was included in his statement.

Rothschild, however, went on to say that in the French periodical *L'Illustration,* for February 14, 1920, page 129, there was a photograph of a gorilla 9 feet 4 inches in height, according to M. Villars-Darasse, "and the photograph certainly shows a gigantic animal" (Fig. 2). A comparison of the size of the gorilla with that of the native sitting alongside it (assuming the Negro to have been 6 feet in height and proportioned accordingly) reveals that the sitting height of the gorilla could hardly have been more than 43 inches. This would denote a standing height of 5 feet 8.5 inches, which is that of a good-sized but not very large gorilla.

Still another coast gorilla of incredible size is mentioned in the French periodical *La Nature,* for July 29, 1905, page 129. This specimen, it is asserted, weighed 770 pounds (!), measured 7 feet 6.5 inches in height and 3 feet 7 inches across the shoulders. Fortunately, the illustrations, which show the dead animal in a sitting posture, show also two adult natives, one standing and the other sitting, which make it possible to check the foregoing measurements. Assuming the natives to have been of average stature, and making due allowance for perspective (as the gorilla is considerably nearer the camera than are the natives), it can be seen that the specimen pictured is little, if any, larger than an average-sized coast gorilla. Therefore, why such impossibly large dimensions should have been claimed for it is difficult to understand. If the publishing of such unconfirmed figures appeared only in the original contribution it would be bad enough, but in this instance the stated measurements were quoted in such standard and justly popular works as Lydekker's *Wild Life of the World* and Brehm's *Tierleben.* Precisely in this way do mere claims or assertions come in time to be assumed as established facts—that is, unless someone goes to considerable length to disprove them.

To digress, it should be pointed out that none of the higher anthropoids (gorilla, chimpanzee, orangutan) can (or at least does during life) stand fully erect as does man. Thus the so-called standing height in apes is really the total length from crown to heel in the *lying* position. In a strictly scientific comparison of the proportions of the body and limbs in man and apes, the trunk length (from sternal notch to pubis) is the most logical and satisfactory basic dimension. Since the height standing is such a familiar measure of size in man, however, the "artistic license" is here taken of considering the gorilla in the same (though, to it, unnatural) posture for comparison.

In a few specimens of gorillas obtained for museums, the skeletons as well as the skins were carefully prepared for mounting. In some of these specimens, measurements fortunately were recorded both in the field (of the external body) and in the laboratory (of the bones of the skeleton). In such

specimens I have checked the dependability of the field measurements by reconstructions of the probable living dimensions of the individual *from its own bones.* By thus using the actual skeletal elements of a specimen, more accurate estimations of its probable size in life are possible than if field measurements alone were relied upon.

A well-known museum specimen of the mountain gorilla, and one for which field measurements have been repeatedly published and quoted, is the adult male (standing figure) in the gorilla group in the Akeley Memorial African Hall at the American Museum of Natural History. This specimen, known as "the lone male of Karisimbi," was shot by Mr. H. E. Bradley, on the Carl Akeley African Expedition of 1921 (Fig. 3).

In publishing the body measurements of this gorilla as taken in the field, Akeley gave the height as 5 feet 7.5 inches and the span, or "reach," as 97 inches. Let us see how these figures compare with a reconstruction of the living dimensions derived from the arm and leg bones of this specimen. The combined lengths of the upper-arm and forearm bones and the skeleton of the hand indicate a living arm length (from shoulder to fingertip) of 1,004 millimeters, or 39.5 inches. The length of the clavicles indicates a living shoulder breadth (skeletal) of 446 millimeters, or 17.5 inches. The living span, or arm stretch, may be derived by taking twice the length of the arm, plus the skeletal shoulder breadth, minus the "loss" in shoulder breadth occasioned by raising the arms to shoulder level (in the gorilla usually about 6 per cent). In the specimen under consideration the span thus derived is 2,307 millimeters, or a trifle less than 91 inches. This—making due allowance for individual variation—is quite irreconcilable with the 97-inch "reach" given by Akeley. A clue to the discrepancy may be found in the measurements of the same specimen published later by Mary Hastings Bradley, wife of H. E. Bradley, and herself one of the members of the Akeley expedition. In this later account, Mrs. Bradley gives the height as 5 feet 7.5 inches (the same as Akeley gave it), but the span as 92.5 inches and the "reach," *from the ground to the tip of the upraised hand,* as 98 inches. If we work backward from the latter figure for the height of the upraised hand (middle fingertip), we obtain an implied stature of 5 feet 5.5 inches. The combined lengths of the bones of the arms and legs imply a stature of 5 feet 5.25 inches. The span (92.5 inches) implies a stature of 5 feet 6 inches. Hence, it would appear that the true standing height of this "lone male of Karisimbi" in life was possibly as little as 5 feet 5 inches, but more probably about 5 feet 6 inches, rather than 5 feet 7.5 inches as published. It should be mentioned that in taking the measurements of standing or sitting height in

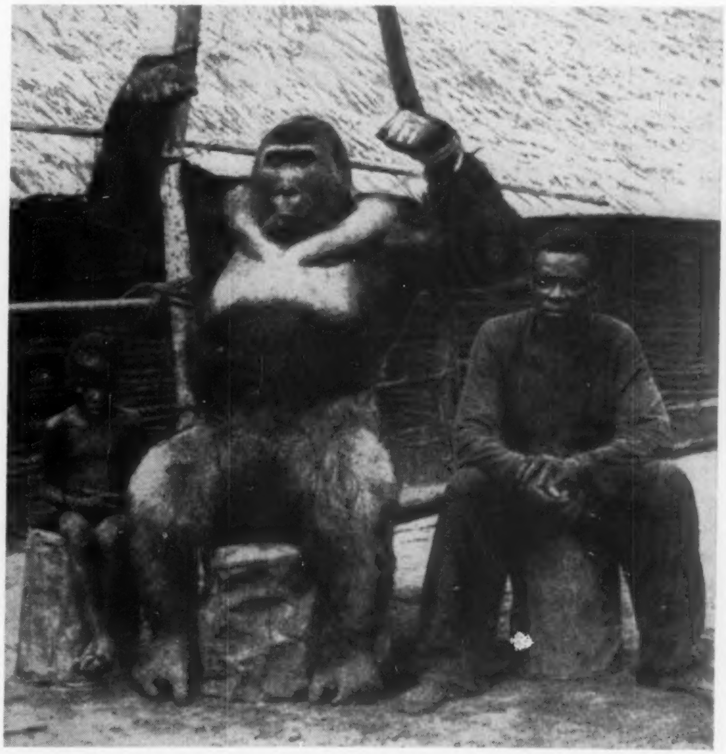


FIG. 2. A fine specimen of the coast gorilla, shot by M. Villars-Darasse in the forest of Bambio, Haute-Lobaze. Although stated to have been 9 feet 4 inches (!) in height, the animal probably stood about 5 feet 8 or 9 inches. (*Photo from* L'Illustration, *Feb. 14, 1920.*)

FIG. 3. "The lone male of Karisimbi," which was shot by H. E. Bradley on the Carl Akeley African Expedition of 1921. From left to right are Mrs. Bradley, Mr. Akeley, and Mr. Bradley. For the author's comments on this gorilla's size, see the accompanying text. (*Photo courtesy American Museum of Natural History.*)

gorillas, the "crown pad" on top of the skull should not be included. This pad of connective tissue, which is probably present in the majority of fully adult gorillas—particularly in males of the mountain species—is often as much as two inches in thickness. If it was included in the height of the Karisimbi male, then, as measured in the field, it alone would suffice to account for the discrepancy in height here noted.

Another seeming disagreement in this specimen is in the weight, which was given by Akeley as "actually" 360 pounds, and by Mrs. Bradley as "nearly 400 pounds." Weight can be closely estimated where the height and girth are known, and the weight implied for this specimen is 437 pounds. This weight, and the height of 5 feet 6 inches, characterize an adult male mountain gorilla of close to average size, rather than "very large," as was at first thought.

More recently, the well-known African explorer, Commander Attilio Gatti, shot an adult male mountain gorilla which was asserted to measure 8 feet 9 inches from the bottom of its feet to the tips of its uplifted hands, and to weigh 531 pounds. These measurements, if correctly taken, indicate a standing height of about 5 feet 10 inches, which is that of a really large gorilla (Fig. 4).

In July 1929, a primate-collecting expedition, under the joint auspices of Columbia University and the American Museum of Natural History, left America for Equatorial Africa under the leadership of the late Henry C. Raven. This expedition returned with three adult male coast gorillas and two adult male mountain gorillas. These five specimens, which were embalmed, were later studied and measured by Dr. Adolph H. Schultz, of the Department of Physical Anthropology of The Johns Hopkins University. Among the many measurements taken by Dr. Schultz, the standing heights (converted from millimeters) of the three coast gorillas were 5 feet 5 inches, 5 feet 5 inches, and 5 feet 7.5 inches, respectively. Of the two mountain gorillas, one stood 5 feet 7.5 inches; the other stood 5 feet 8 inches and weighed 467 pounds.

In 1934 the Vanderbilt Expedition of the Academy of Natural Sciences of Philadelphia obtained in the French Cameroons two adult male gorillas and one immature male. The larger of the two adults is the standing figure in the gorilla group in the Museum of the Philadelphia Academy of Sciences. This specimen stood in life about 5 feet 7.5 inches and weighed just over 500 pounds. Another adult male, which was obtained in the most eastward location yet recorded for a living coast gorilla (namely, in the forest about 15 miles east of the Sanga River and 22 miles northeast of Nola), had the longest limb bones on record and, although not quite as heavy as the aforementioned specimen, must have stood in life over 5 feet 9 inches in height. A field photograph of this truly enormous coast gorilla is here reproduced (Fig. 5. From Coolidge, Harold J., Jr. *Proc. Acad. Nat. Sci. Phila.*, 1936, **88**, 479–501).

The late T. Alexander Barns, an English natural-

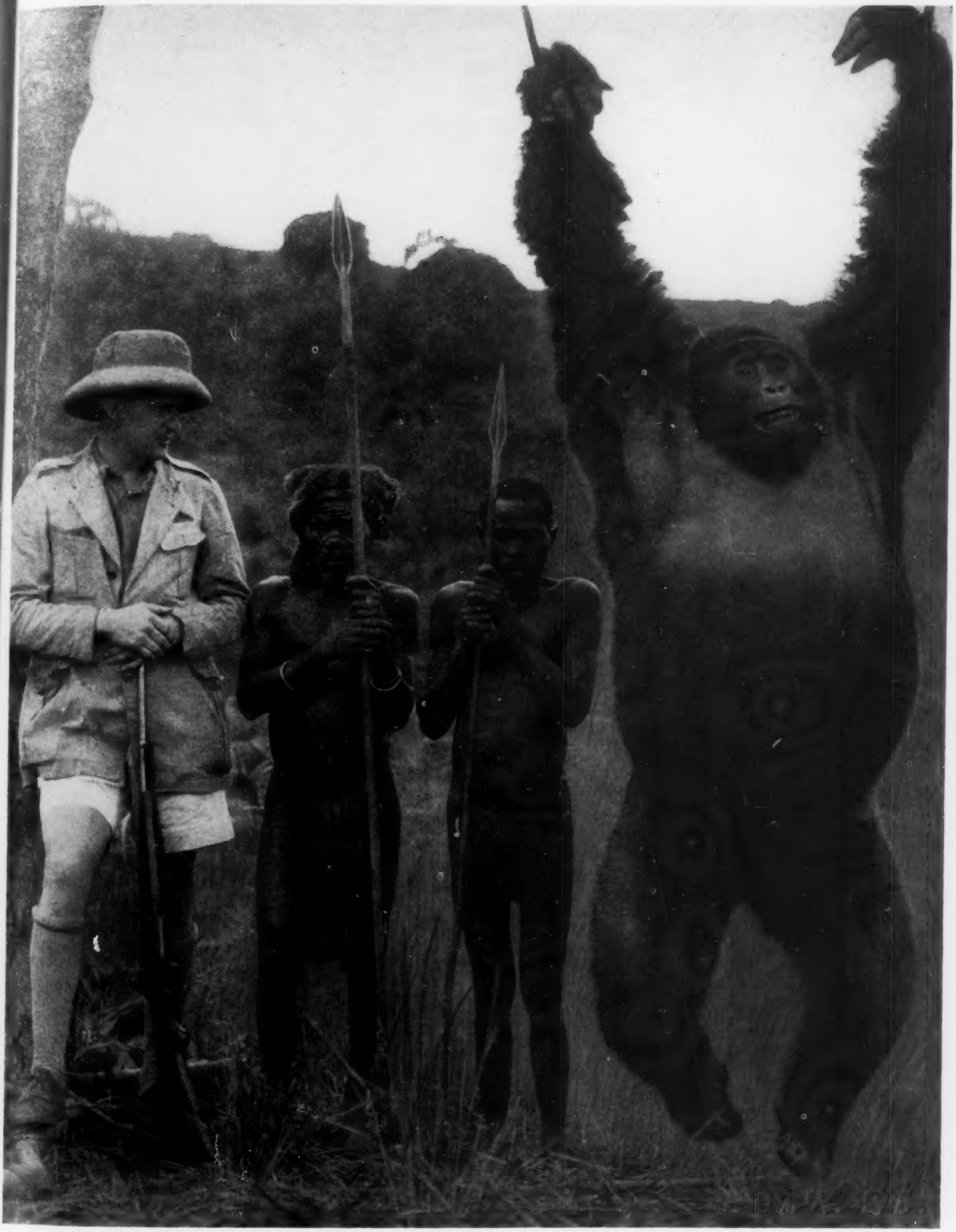


FIG. 4. A very large mountain gorilla shot by Commander Attilio Gatti (*at left*) in 1930 for the Museum of the Royal University, Florence, Italy. Next to the gorilla are two pygmies of the Mambute tribe, who located the giant specimen in the forest of Tchibinda, near Lake Kivu. (© *Gatti-Hallicrafters Expedition.*)

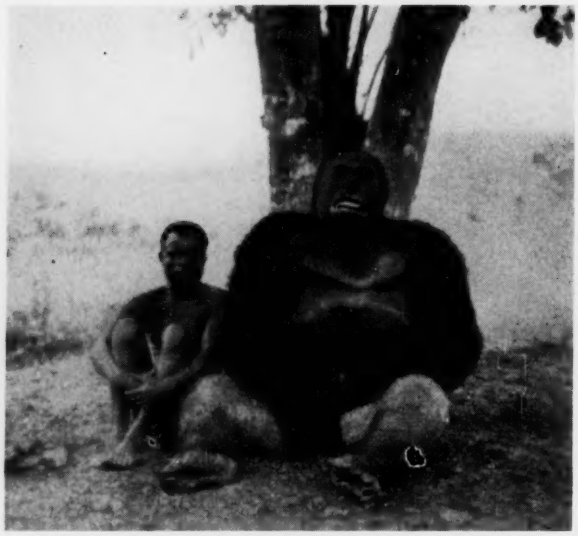


FIG. 5. Very large male coast gorilla secured by the Vanderbilt African Expedition of 1934. This specimen was found in the most eastern locality yet reported for a coast gorilla, namely, on the east bank of the Sanga River in French Equatorial Africa. (*Photo courtesy Academy of Natural Sciences of Philadelphia.*)

ist-explorer of the Belgian Congo, has stated: "In spite of exaggerated reports which I have before me of Mr. Howard Ross's supposed discoveries in Sierra Leone of a 9-foot gorilla, I am quite certain that these splendid apes never attain a standing height of more than 7 feet—if that! The largest one shot by the writer [T. A. Barns] measured 6 feet 2 inches from heel to crown, and I believe it to be a record measurement." Barns' gorilla specimens were all of the mountain species (Fig. 6).

Another gorilla (coast species) of exaggerated size is shown in Figure 7. This specimen was stated to be 7 feet in height and 40 inches across the shoulders!

Concerning living, captive gorillas, there have been between thirty and forty individuals imported to the United States since 1897. In that year the first, an infant male, was brought from Liverpool, England, to Boston, but survived in this country only five days. Of the specimens imported during the years since then, many, despite every care, have died. Included were the two very large, nearly adult mountain gorillas of the San Diego Zoo, which had been carefully reared since the time they were five years old. On the other hand, though no gorilla has ever been born in captivity, some five or six individuals have been successfully reared from infants to full-grown adults. At present, perhaps the finest specimen in captivity anywhere is the huge coast male, "Bushman," of the Lincoln Park Zoo, Chicago. Bushman, who is now more than twenty-two years of age and quite adult, weighed at last report 550 pounds. His height is said to be about 6 feet 2 inches. This, if substantiated, would make him the tallest gorilla, of either the coast or the mountain species, ever seen in captivity. But it can readily be understood why, after a growing male zoo gorilla has passed the juvenile stage, the only measure of his size that can be directly taken is his weight. However, there is an adult female gorilla—"Susie," of the Cincinnati zoo—who has been measured with the tape, as well as weighed, once each year since 1936, by her trainer, William Dressman. These yearly lists of measurements of Susie have been invaluable to me in my determination of the typical relationships between living height, girth, and weight in female coast gorillas.

Dr. Adolph H. Schultz, who has perhaps measured more embalmed bodies and skeletons of anthropoid apes than any other man, has contributed enormously to the knowledge of the physical structure and bodily proportions of the gorilla, chimpanzee, orangutan, and gibbon. From the dimensions of the limb bones of no fewer than ninety-three adult male specimens of the coast gorilla as measured by Dr. Schultz, it can be deduced that the average living standing height is 1,600 millimeters, or 5 feet 3 inches. Similarly, the living height of the smallest male gorilla in this skeletal series can be calculated as about 4 feet 10 inches, and the largest, 5 feet 9.25 inches. The average adult female coast gorilla stands 1,342 millimeters, or about 4 feet 4.75 inches; and in Dr. Schultz' series of sixty skeletons the smallest female was in life about 4 feet 1.5 inches, and the largest, a fine specimen in the American Museum of Natural History, about 4 feet 9.25 inches.

As a matter of interest, I have taken the ranges in limb-bone measurements shown in the aforementioned series of ninety-three male gorillas and sixty female gorillas and statistically extended them to cover 10,000 cases of each sex. This number should provide a fair idea of the probable range in stature of the entire adult coast gorilla population. In this hypothetical population of 20,000 wild adult gorillas, we would expect the tallest male to be about 6 feet 2.5 inches, and the shortest male, 4 feet 3.5 inches. Similarly, we would expect the

FIG. 6. The tremendous bulk of a full-grown male mountain gorilla, in comparison with a Watussi native 5 feet 10 inches in height, is shown in this photograph of a specimen shot by the late T. Alexander Barns in the Lake Kivu region of central Africa. (*From Barns, T. A.: Across the Great Craterland to the Congo. London: Ernest Benn, 1923.*)

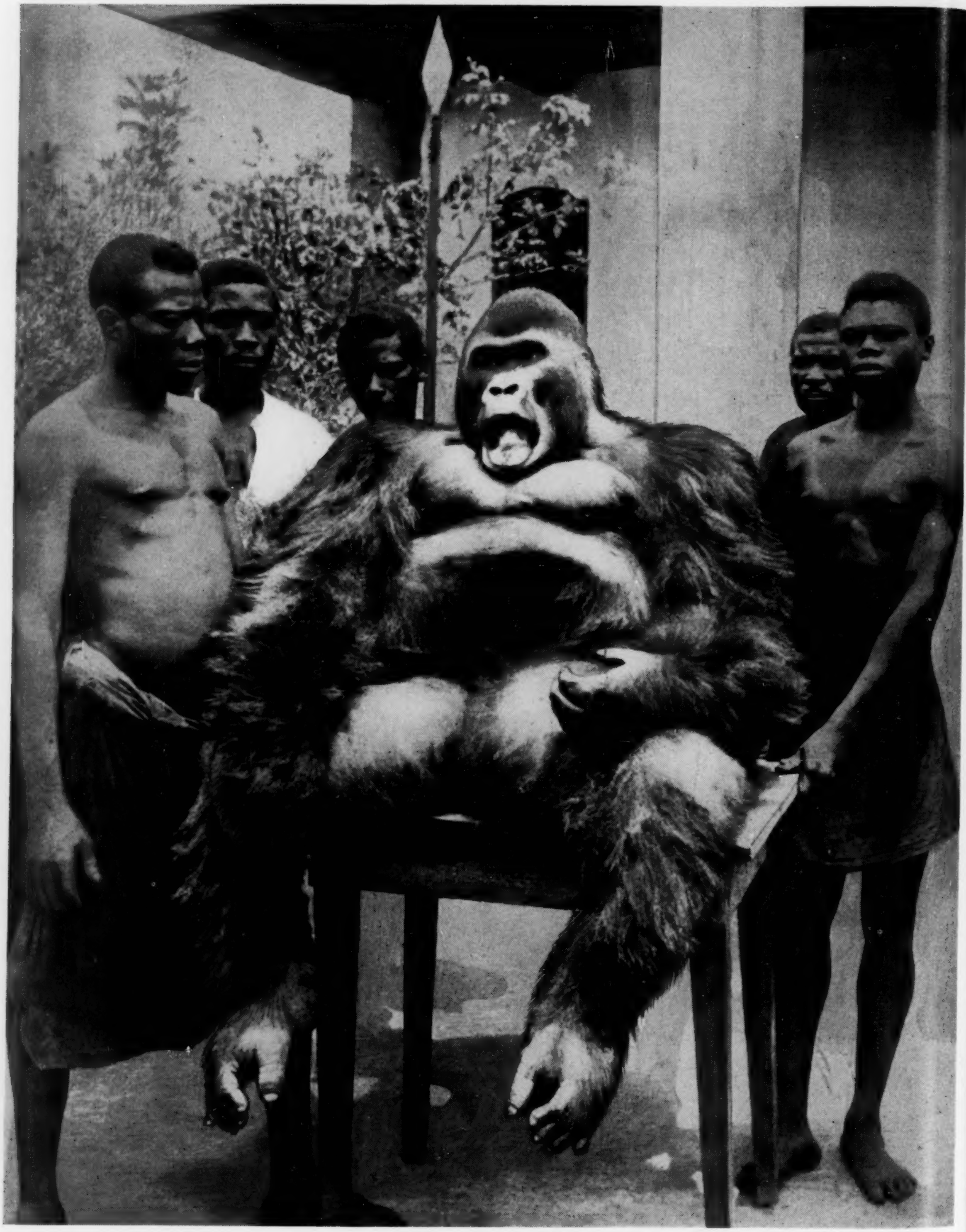


FIG. 7. Here is a spectacular field photograph, taken some years ago, of an adult male gorilla killed in one of the forests of the Cameroons, West Africa. The specimen appears, however, to be of little more than average size, and far short of the "7 feet in height" announced in news dispatches at the time. (*Press Association, Inc.*)

tallest female coast gorilla to be about 5 feet 1 inch, and the shortest female, about 3 feet 9 inches. Respecting the tallest female, it is interesting to note that the female coast gorilla Susie, of the Cincinnati Zoo, actually attained, at the age of seventeen and one-half years, a height of 5 feet 2 inches and a weight of 405 pounds. It is known, however, that some zoo animals (because of better food supply) attain larger size than wild specimens of the same species.

The heaviest, though not the tallest, mountain gorilla was the late "Mbongo," of the San Diego Zoo, whose highest recorded weight in life was about 660 pounds. Complete measurements of the skeleton of this specimen were taken by the writer in the fall of 1942. Mbongo's height, in life, was 5 feet 7.5 inches.

The standing height of the mountain gorilla is, on the average, 5 feet 6.25 inches in males and 4 feet 7 inches in females. The weight in the adult male averages 458 pounds and in the adult female 240 pounds. In the coast gorilla the average weight is 344 pounds in the male and 188 pounds in the female. These weights are for wild, active specimens.

Dr. Schultz, after finding numerous constant differences in head, body, limb, hand, and foot proportions between the bodies of the coast and the mountain gorillas he studied and measured, concluded that the mountain gorilla is specifically distinct (that is, a different *species*) from the coast gorilla, and with this conclusion I concur.

Although the maximum individual standing height of the mountain gorilla (because of being drawn from a smaller population) probably does not appreciably surpass that of the tallest coast gorilla, it is significantly greater on the average, as are also all the measurements of girth and, accordingly, of weight. Thus, of the two species, the mountain gorilla may be said to be typically the taller and larger animal, with proportionately thicker neck, chest, and limbs. Other differences in the mountain species are, in relation to height: a longer trunk (sitting height), shorter arms (span), broader shoulders, and more tapering limbs (from thigh to ankle). The comparatively massive neck of the mountain gorilla is especially notable. An interesting detail is that although the span, or horizontal arm stretch, is comparatively less in the mountain gorilla, the standing reach upward is about equal to that of the coast species. This is due to the shoulder joint in the mountain gorilla being relatively higher than in the coast gorilla, thus enabling the whole arm to be raised higher.

To conclude, and to answer our opening question, it can be said that a standing height of over 5 feet 8 inches in an adult male gorilla of either the coast or the mountain species denotes a "large" specimen; over 5 feet 10 inches, a "very large" specimen; and 6 feet 2 inches, the greatest height reliably recorded. Many a man is taller while standing than even the tallest gorilla, but the greater trunk length and girth, the incomparably longer and heavier arms, and the more robust and massive skeletal make-up of the gorilla make it, as a species, outstandingly the largest living primate. Indeed, it is a worthy rival, physically, of the most gigantic fossil apes or men.

Fishing in Arabia

DONALD S. ERDMAN

The author, scientific aide in the Division of Fishes, U. S. National Museum, was a guest of the Arabian-American Oil Company (ARAMCO) at Dhahran, Saudi Arabia, from March to August 1948. ARAMCO was carrying on a local fishery survey to supplement the food of company employees. Mr. Erdman assisted in this project and made a representative collection of 5,000 fishes and other marine life from the Persian Gulf. He also made a trip to Jidda on the Red Sea in July and another to the interior oasis of Al Hasa.

"Samak, waajid!" ("loads of fish"), the Arab fisherman said to me as he spread wide his arms to indicate size and quantity. Later, experience proved that the fishes were often fewer in number and smaller in size than I had been led to believe. But fishermen and fish stories are the same the world over.

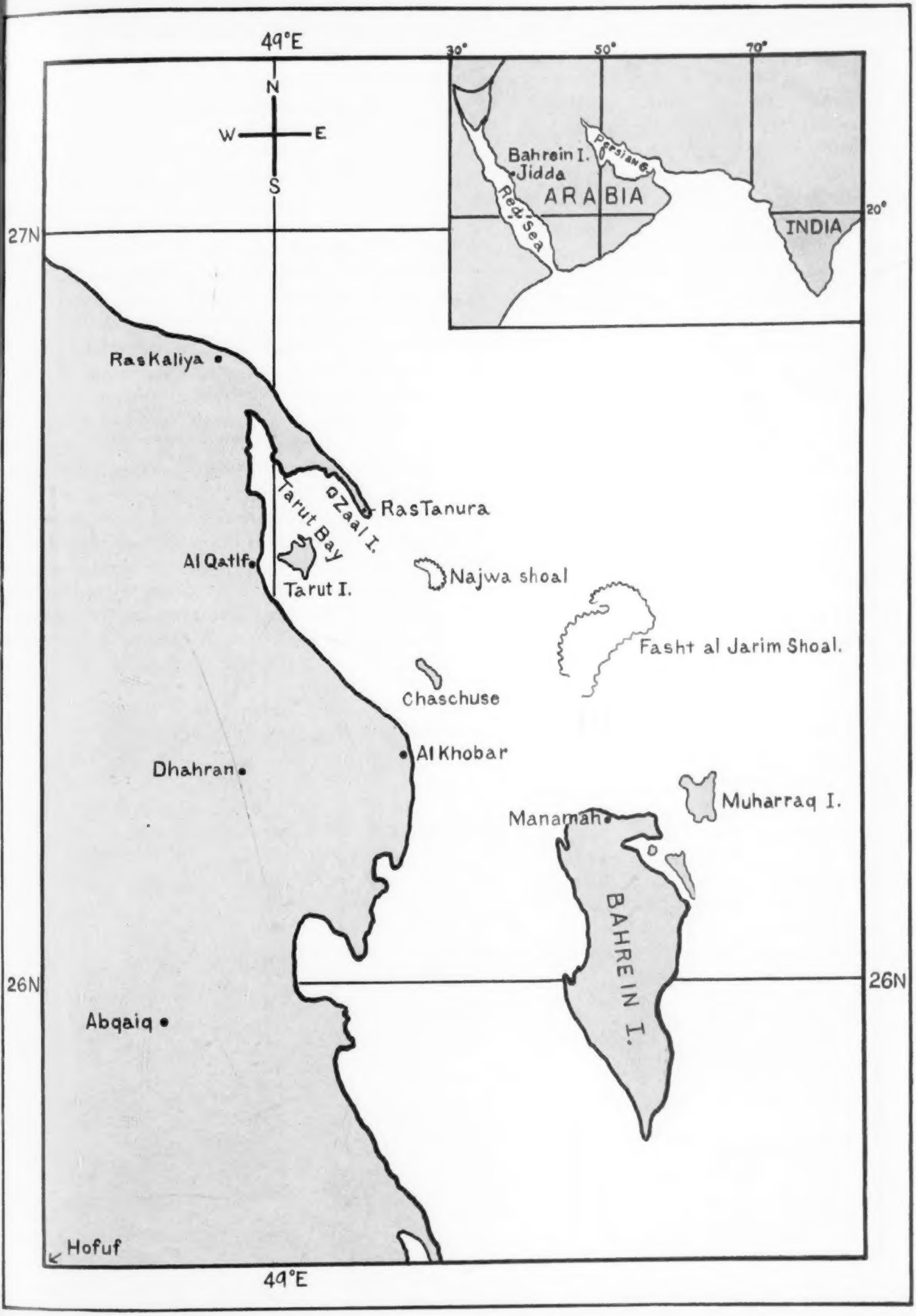
Enthusiasm for, and skill in, hook-and-line fishing know no limits in Arabia. Every evening one of our launch crewmen would squat on the after rail and cast his line far out, barely moving it back and forth between his thumb and forefinger. He averaged about two fish every five minutes during the time he fished, and he never failed to catch enough for his supper. A familiar sight on the pier at Ras Tanura is a ragged old Arab, with a small hook and hand line, outfishing an American right next to him with a ponderous surf-casting rod and highly polished reel. With their threadlike lines and tiny hooks, the Arabs catch the halfbeak, or *sils,* a bait fish, and even the large porgy, *subaitee.*

Fishing for the subaitee, *Sparus cuvieri,* is distinctly an art. This fish is an ordinary porgy, but with special habits. If you throw bread upon the water from a pier, large subaitee appear from the depths to take it, just as tame carp do, and in this manner they will take anything, with one exception —they are allergic to fishhooks. Only a person of skill and experience can hook a subaitee. A small Arab boy fishing one morning off the bridge between Muharraq and Bahrain had a basketful of small fishes, a thin cotton line, and a little hook. The tide was rushing in under the bridge, and subaitee were darting around barely keeping up with the current. He carefully baited his hook, so that all of it was buried in fish; then he threw a large handful of whole fish and fish pieces into the water and soon after cast the baited hook into the center of the floating pieces. Instantly several subaitee rushed up to the surface to devour every bit. After about ten such casts without success, a subaitee finally ran off with the hook. The boy went quickly over the bridge railing and, climbing down the rocks to the water's edge, carefully pulled the fish toward the bridge and skillfully captured it.

Hospitality is the byword in all Arabia, on land or sea. During a blustery shamal, or north wind, we visited a stake-net fisherman to purchase bait. Invited aboard to pass the time of day before discussing business, we were offered fresh roasted fish and shrimp cooked over a wood fire in a small sandbox on deck. A rough awning of battered canvas was our only shelter from the wind. Bait was nowhere in sight, but in due time we learned that mole lobsters and crabs were living over the ship's side in grass bags.

Another day I went fishing in Bahrain with an Arab fisherman. Propelling his 20-foot boat by means of a long, iron-pointed pole with a barb at one end, he would stop occasionally to dive over the side and pick up a fish trap containing several small porgies and *Siganus siganus, sawfee sunaifee.* For bait he pounded a green sea-lettuce similar to *Ulva* with stones and made a sort of ball, which he placed in the traps. Later he picked up from the bottom dead fresh porgies killed by *sim,* a native fish poison.

Many fishermen are pearl divers at one time or another during their lives, and so swimming and diving become an integral part of fishing. Commonly used in Tarut Bay and Bahrain are handmade wire fishpots, *gerugir,* which are set on the bottom and recovered by a diver. Lines and floats are unheard of because of the extra expense (and mistrust of one's neighbor). The traps are set in a line without regard to landmarks, of which there are, in any case, very few. Our skipper set six traps at Najwa Bank, but because of a shamal we were unable to pick them up until a week later. When we returned and dropped anchor, he recovered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four traps in half an hour and said the other two had been stolen. Except for one stationary marker on Najwa Shoal more than half a mile away, there were no points of reference—just water everywhere and extremely hazy land in the distance.

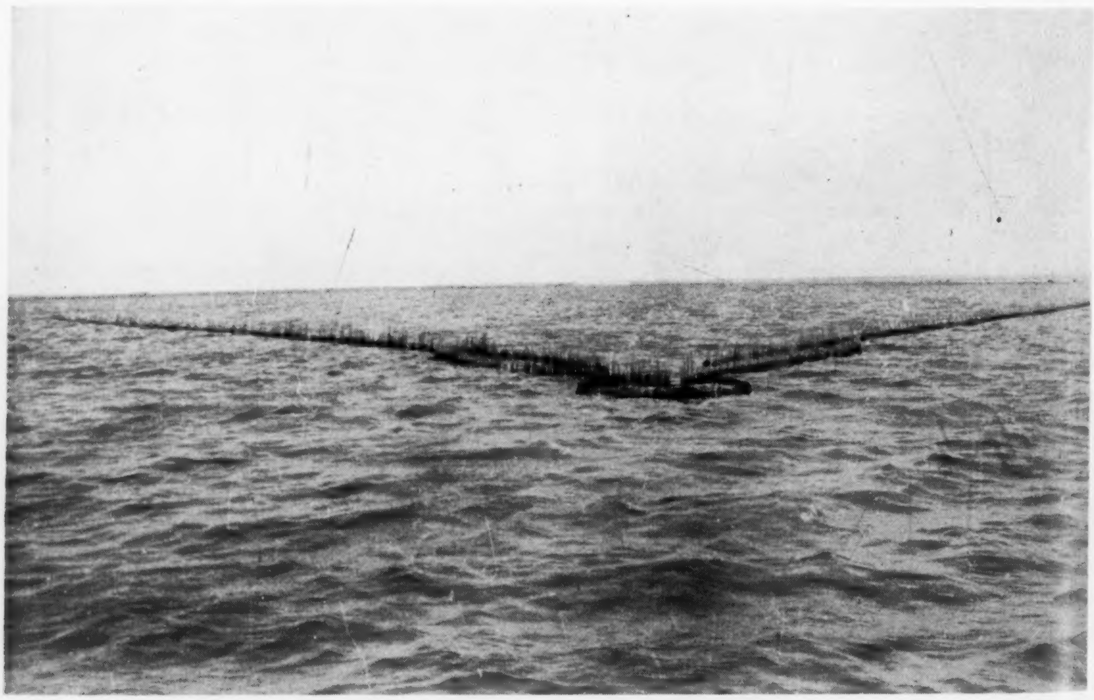
Palm-stake traps are popular in the Tarut Bay and Bahrain area. The strong stems of large date palm leaves are stripped of their leaflets and the ends stuck in the muck sand, one stem lashed closely to the next until a whole trap is formed. These traps operate on the principle that fish swim inshore with the high tide and go out when the tide ebbs. At high tide the tops of the palm stakes are almost under water, but at low tide they are readily visible. In Tarut Bay the traps are arranged in a wide V, the open part toward the shore. At the angle of the V is a rounded trap box from which the fish are recovered. The fisherman gets down into the trap with the fish and wades after them with a short, square, seinelike net with two wooden handles. In Bahrain the traps have a single leader perpendicular to the shore, and they are probably more effective in catching fishes swimming in schools parallel to the shore. Shore seines, cast nets, and hook and line are also used.

Fish drying is widely practiced. Our crew would often split and dry fish, fish heads, and cuttlefish in the rigging or on the deck roof. Salt was seldom used, the drying being accomplished by direct sunlight. Commercial dried fish at Manama Pier on Bahrain Island are as pliable as wood shingles, but slightly tougher. Even so, I saw small boys walking nonchalantly along chewing on dried fish as if it were candy.

Trolling with feathers and jigs is good sport in both the Persian Gulf and the Red Sea. The king mackerel, or *chanad* (*Scomberomorus commersonii*), was our most common catch during April and May. Barracuda, jacks, and groupers may also be caught in this way. Bluefin tuna are caught by rod and reel at Ras Tanura. Large schools of bonito or small tuna were seen cutting the surface around Ras Tanura during May and June, and I once saw several marlin jumping out of the water about three miles offshore.

The Ocean Community

Fishing with a 100-watt bulb over the side of a boat at night with a crab or hoop net was a successful method of catching many types of ocean dwellers. A panorama of sea life passed by the light each night: fishes, sea snakes, crabs, mole lobsters, shrimps, sea worms, and, occasionally, a cuttlefish or a squid. Most abundant among the fishes were round herrings, sardines, halfbeaks, silversides, and billfish.



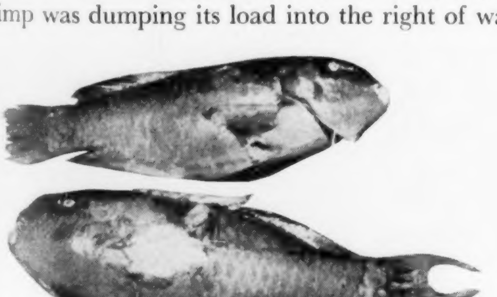
A palm-stake fish trap off Tarut Island.

Sea snakes (Hydrophidae) one to three feet in length often came to our light in May and June, slowly wriggling their way to the surface from the bottom, just as a snake goes through grass, only more leisurely. On reaching the surface, the snake gulps air and descends rapidly, the period of time that it has its head at the surface being always very short. All sea snakes are poisonous; they do not attack bathers in the water, however, and are sluggish out of water. They are white, with dark bands, and have rounded and flattened tails to aid in swimming; they feed on bottom-dwelling fishes.

The mole lobster (*Thenus orientalis*), white in color and ten to twelve inches in length, swims at the surface at night—backward with rhythmic flips of the abdomen. The tail meat of these lobsters (*mu al rubyan*, "mother of shrimp") is very sweet—tastier and tenderer than that of the spiny lobster. We caught them with crab nets, flipping them up to the roof of the deck, where they would rattle around until we grabbed them.

Female crabs (*Portunus*) with gray eggs, were common at the surface, but were wary of the light and net and could change direction abruptly. We once caught snapping shrimp at the surface also, although this shrimp lives in coral-rock holes on the bottom. On another night large schools of the three-spined foolfish (*Triacanthus indicus*) came to the light, and the crew had a riotous time scooping them up on deck. More than 100 fish eight to ten inches long were caught in an hour's time in this way.

Then there is the "bulldozer fish," a molelike shrimp that excavates burrows two feet or more in the hard clay sand below the low-tide mark. Every few minutes the shrimp rushes from its burrow, shoving a pile of sand at least six inches away from the entrance, disappearing immediately thereafter backward into the burrow, only to return a moment or so later with another load. I watched two "bulldozers" working at right angles to each other in about four feet of water. One shrimp was dumping its load into the right of way



Arab fisherman, on Tarut Bay, exhibits prize *hamoor*.

of the other, but both were so busy bringing out sand that neither realized what was happening. Associated with each "bulldozer" is a blenny or, what is more usual, a large banded goby (the kind the sea snakes like), which sits by the side of the burrow, apparently oblivious to the frenzied activity of its housemate. When danger threatens, this "watchdog" plunges into the burrow, to appear later when the coast is clear.

One night, as we were seining along a sand beach near Ras Tanura, Swede Morrill, a husky six-footer, stepped on a tiny scorpion fish, not more than an inch and a half long. Immediately, a stinging, agonizing pain, lasting about half an hour, worked from his foot up into the groin. According to the Arabs, this fish, the *faryalah,* never exceeds six or eight inches in length, but it is much feared and respected by them. I later watched one wriggle itself down into the wet sand, its front dorsal spines erect and its pectoral fins spread to give it firm support.

Just as the land has its seasons, so does the sea. Rooted beds of gulfweed were abundant around Zaal Island, Tarut Bay, during April and May 1948. At low tide the brown tops would lie limply at the surface, and it would be impossible to row through them, although these gulfweed areas

Arab seamen from boats anchored in the harbor race toward shore at Manama, Bahrain.

were favorable for the collection of small fishes. During a visit to the same spot on June 1 not a single piece of gulfweed was in sight. At first I thought I must be in the wrong area, but eventually I discerned the remains of small black rootlike structures on the bare, coral-rock bottom. Thus a whole gulfweed habitat had been wiped out almost overnight.

The Persian Gulf

Any description of the Persian Gulf in summer without the mention of jellyfish would be incomplete. I saw myriads of large white jellyfish eight inches to a foot in diameter near Al Khobar one day in June. A solid belt of them over 100 feet wide stretched north and south as far as the eye could see. On each side of the jellyfish belt more individual jellyfish were going by our boat than one could possibly count. This species is a solid white mushroom medusa with no long tentacles but with three- to four-inch egg masses hanging underneath the body. They are efficient swimmers; I have seen them bump against dock pilings and simply turn around and swim off against the tide without any difficulty. Greatly feared by pearl divers is the *dool,* a small bell-shaped, opaque-tan medusa with colorless tentacles two to three feet in length. The *dool* swims at any level in the water and cannot always be readily seen. A tentacle touching a diver's eye could cause blindness. At Ras Tanura the *dool* is most abundant about one hour before flood tide—a good time not to be swimming.

In his *Fishes of the Iranian Gulf,* H. Blegvad listed a total of 215 species. Of these, 90 are known to range from the Indian Ocean to the Philippines, and 5 range from the Persian Gulf to South Africa. Twenty-five are rather local, 5 are cosmopolitan species, and 90 occur otherwise only in the Indian Ocean. In my own collections there are about 30 species of which Blegvad was unaware at the time his account was prepared. Their distribution pattern corresponds closely to the one pictured by him. Thus, the Persian Gulf would appear to be largely populated by Indian Ocean species or forms closely related to them, although it has relatively few species compared to that ocean. It can be safely estimated that the number of species in the Indian Ocean would approach 1,000. Despite the relatively small number of species in the Gulf as compared with the Indian Ocean, these species are present in greater numbers than one might expect in such a "hot" body of salt water, where organic material is rapidly oxidized. Best represented of the several families of fish present are the porgies, Sparidae, of which there are at least 10 abundant species in the Gulf. On the other hand, the parrotfishes, which are so numerous in point of species in the related Indian Ocean and Red Sea faunas, are represented by but a single species.

Recent investigations of Pacific and Indian ocean fishes have shown that many fishes formerly considered one species over a large area may actually be two or more; nevertheless, a number of very closely related fishes are found over a great expanse of ocean.

The Persian Gulf is a shallow arm of the Indian

Ocean; its greatest depth is less than 50 fathoms. Owing to rapid evaporation in summer, the salinity is high. In Tarut Bay the bottom consists of a fine gray, clayey sand, sand, calcareous rock,* and scattered patches of live coral, a brain coral and a branching form, *Acropora.* There are no pavementlike coral reefs in this part of the Gulf, and species of brightly colored coral-reef fishes are few. During storms the water becomes roiled and opaque gray in color as the result of the disturbance of the clayey sand bottom. In water this sand looks and feels like a gray mud, but if allowed to dry on an anchor it becomes white, extremely hard, and rough to the touch rather than smooth.

At Jidda, on the Red Sea, there are two long series of coral reefs that parallel the coast for several miles, separated from each other by an expanse of deep water, and Jidda Harbor is studded with many individual coral heads that come within a foot of the surface. The Arab sailors have learned to know where the reefs are and can sail safely by them in the dark.

Many of the Red Sea corals are bright pink and green, and the reefs abound with fishes of jewel-like colors. At the edge of the offshore reefs, the water deepens into a rich blue, and large fishes cruise leisurely about. The reefs are ideal for spear fishing, using a glass face mask.

At Sharm Ubhar, about 10 miles north of Jidda, an arm of the sea enters the coast line, and the small bay here is surrounded by massive brown coral rock on either side. Each rock is a chronicle

* The coastal Arabs use slabs of the calcareous "sea rock" in building their houses. The donkey drivers with their carts wade out for a half mile or so at low tide at Manama to pick up loads of "sea rock" and trudge back to shore.

Fishermen rowing in Tarut Bay. Note how they sit on the gunwale and bend out with each stroke of the oar.

A *jalboot* under way in Tarut Bay. The word "jalboot" is thought to be a corruption of the British "yawl boat."

of the corals that have lived in the distant and the more recent past. Most of the bottom is underlain with sharp coral rock, and the beautiful *Tridacna*-like clam lies attached to the bottom, its shell open and showing an indigo mantle that can turn to bright yellow in an instant. This rugged bivalve will close on an unwary passer-by.

South of Jidda are extensive mud flat areas behind the reefs. These flats are inhabited by crabs and small fishes such as gobies and killifishes (*Aphanius dispar*), but the mud is so underlain by sharp coral that wading barefoot is dangerous.

Weather and Tides in the Persian Gulf

In summer the Persian Gulf is one of the warmest bodies of salt water in the world, with the Red Sea a close second. The bordering land is all sunbaked desert, under an almost cloudless sky. Air temperatures at Bahrain Island reach 109° F. in summer and 41° F. in winter. One day in August, while wading near the shore of Chaschuse Island in uncomfortably warm open water, I recorded 39° C, or 102.2° F.

The Arabian desert is noted for its strong northerly winds, the shamal. In early summer these are hot, dust-bearing blasts that blow several days at a time, then cease for two or three days, only to resume for another interval of several days. (The summer shamal is sometimes called the forty-day shamal.) Even after a shamal has ceased blowing, the thick brown dust hangs in the sky over the Gulf, cutting visibility down to less than a quarter of a mile for several hours, or even a day. In winter the shamal brings cold air and near-freezing temperatures. Sometimes, during extreme cold spells,

fishes are killed that later drift onto the beaches.

The tide at Ras Tanura varies from 5 to 7 feet. At high tide most of Tarut Bay is navigable for small boats of 3- to 4-foot draft, but at low tide there are miles of extensive clay sand flats, and boats lie over on the bare sands, except in a few channels of deeper water. One evening in April after we dropped anchor half a mile offshore from Qatif I arose during the night to discover that the stern was high and dry on a sand bar. The following morning land was once again half a mile away.

Tidal currents at Ras Tanura and in Tarut Bay average 4 knots or more. In certain places they are so swift that a boat at anchor will lie in a cross sea, even when the wind is very strong. The period of slack water at ebb and at flood appeared to be short, which is a disadvantage for both night-light and hook-and-line fishing. Swift tides occur all along the coast and all the way to Bahrain Island.

I was in Jidda a week before I could detect any tidal change in the Red Sea, but one evening I noticed that a small mud pool was connected with an inlet; until then it had been separated for days by a few inches of beach. Records of the International Bechtel Company show an average tidal change of 6 inches over a six-month period, with a high tide in January of 2.6 feet.

In Dry Dock

Dry dock in Manama is elemental (no iron ways, no winches, no boat cradles)—at high tide you just sail your *jalboot* up to a section of beach allotted for that purpose north of Manama pier. Poles are lashed on either side of the bottom of the boat so that when the tide goes out the vessel will stand on an even keel. Caulking or barnacle-scraping is then the order of the day, and eventually the application of a white paint on the bottom by hand. This paint has chiefly a lime base. The topsides and deck are treated with raw fish oil, applied with a rag or merely the bare hand.

About six o'clock the first morning in dry dock there was a series of *Kaif Halechs* and *Salaams* and other greetings, so I realized that someone not a member of the crew was boarding our boat. First, a weird, ancient box came up, filled with tools with which I was unfamiliar. Next I beheld a bearded one-eyed old man attired in dirty gray cap and gown. He was thin and gaunt, surely a runner-up for Methuselah, but his manner was pleasant, and, like all Arabs, he proved to have a sense of humor. This was our ship's carpenter—respectfully referred to by Ahmed, our cook, as *Nakhoda* ("captain")—ready to go to work on our leaky deck with wooden hammer, crude chisel, coping saw, and a bowstring drill (which reminded me of the apparatus used by enthusiastic boy scouts to make fires). Even caulking irons in Arabia have a different edge, sharper and narrower than those commonly used in North America. Untreated cotton was used for caulking the deck.

The carpenter would squat barefooted, his usual working pose. If a board with a soft spot was found on the deck, his technique was to chisel out a hole around the spot until good wood was touched. Then a new piece was cut with the coping saw to fill in the hole. Wood is scarce in Arabia, so it must be conserved. With good luck a single piece of wood could last aeons of time, since the boat would continually be built around it. In fact, even new boats are pieced together like jigsaw puzzles. Their lines, however, are good. After several mornings of industrious labor, the work of the ship's carpenter was deemed finished, and the deck was greatly improved, comparatively speaking.

Living on the boat for a week in dry dock was a great experience, since all my actions were noticed by passers-by on the waterfront. At first I was a little annoyed and self-conscious, but I soon became so readily accepted by passers-by and visitors alike that life became almost humdrum. Nearly all the Arabs I met showed one paramount virtue, that of accepting people, especially strangers, as they are and not being inquisitive or amused in an unkind way. One or two exceptions stand out only because there were so few. But an American living on an Arab dhow in dry dock, writing voluminous notes and pickling good fish in an evil-smelling fluid, would be a lot for anyone to take. Nevertheless, Arabs came and went and talked with me with no evidence of concern or alarm. The basis of their calm is their faith in God. *"In sha Allah"* ("as God wills") is on the lips of a Moslem many times a day, and so the coming of myself, or even the oil company, to Arabia was no surprise. Allah willed that it should be so, and it was so.

The Fresh-water Springs of Al Hasa

The great oasis of Al Hasa is 50 miles inland from the Persian Gulf. The largest town is Hofūf, with about 10,000 inhabitants. Here beneath the shade of more than a million date palms are clear, cool springs that well up out of the ground and flow for a mile or two as small rivers, only to disappear again as mysteriously as they first appeared. The water is crystal clear and sweet-tasting, even during the hot days of August. Although the air

Ain, fresh-water spring at Hofũf. Rice paddy visible in the background.

temperature was usually well over 100° F. in the middle of the day, the highest temperature that I recorded in any of the springs was 89° F. Besides supporting an extensive agriculture of dates, rice, figs, limes, and other crops, the waters have a life of their own, including two kinds of fish, turtles, frogs, many aquatic insects, snails, leeches, and lush aquatic plants.

The fish in these springs are small killifish (which are also common along the coastal areas of Arabia in both fresh and brackish water), and a fresh-water mullet (Mugilidae), which does not exceed five inches in length. The killifish vary greatly in size, shape, and color, which is not true of this species that lives in salt water. The mullets are tan above, with three diffuse lengthwise stripes on the sides, and white-silver below. The fin rays of many of the mullets examined were crooked, but the fish were healthy and normal in all other respects. Both species of fishes are dried and salted to some extent by the Arabs for use as food.

Hofũf, where I stayed for several days, is a walled town built of sun-baked mud. Arab sentries guard each gate. Americans are admitted on business as friends today, whereas a few years ago they were unknown there. All business is transacted in the open market place, where camels groan and late-model motor trucks rumble on their way; the ancient and the modern live together in apparent harmony. The palace of the Emir of the Province of Al Hasa—to which we paid a courtesy visit —although lighted by electricity, had guards equipped with ancient muskets and long sabers in decorated silver scabbards. Century-old customs still persist in Hofũf, in spite of modernizations here and there.

It was a never-to-be-forgotten experience to spend these few months learning to know the Arabs, their fishing methods, and the native fish of the region. All in all, we had a most successful time charting the occurrence and abundance of the various kinds of fish and their availability as an increased source of food for both the native population and the influx of "foreigners" developing the oil fields.

The good will displayed by the Arabs where the Americans were concerned meant a lot to me; I hope they were equally impressed by us.

SCIENCE ON THE MARCH

BENCHMARK

THE 1947 Census of Manufactures now being made available is the first one taken since 1939. This gap between 1939 and 1947 represents the longest one without an accurate industrial inventory during this century; it was also the eight-year period in which the greatest changes took place. Because no comprehensive fact-gathering was done during the war, the 1939 Census of Manufacturers had to serve as a point of orientation and departure for those who needed to judge what was happening in this important segment of our economy. Although the new census is not yet available in its entirety, some of the data already released lead to interpretations at variance with notions we have become accustomed to hold.

Of facts so far made public, perhaps the most important are those that show changes in geographical intensity of industry from prewar to postwar. What were the major industrial districts in 1939? What are they now? How do they compare? What areas grew most, percentage-wise? In absolute quantities? Was there marked migration to the South and West Coast? Did the heavily industrialized Northeast suffer out-migration? But let us get on to a look at the facts.

The map (Fig. 1) tells most of the story. It shows the uneven arrangement of industry over the land. Fine stipple at the base of bars indicates areal extent of major industrial districts. The measure of industrial intensity (lengths of bars) is based on number of factory workers employed in these areas by business establishments primarily concerned with giving form utility to raw materials. Small industrial districts having less than approximately 35,000 production workers in 1947 are omitted; areas shown account for about 60 per cent of all factory workers in the United States.

One of the most obvious facts shown by the map is the nearly complete absence of important industrial districts in great stretches of our country. Except for marginal areas and the Mohawk Trough, the Appalachians extending from northern Alabama to Maine stand out as a great slab of rough terrain without major industry. The South, even including Texas, makes a weak showing. Principal regions of the West—Great Plains, Rocky Mountains, dry Intermontane Plateaus, Sierra Nevada-Cascade Mountains—are all blank on the map; of the four major districts along the West Coast, only the Los Angeles area makes a creditable comparison with centers of the Northeast.

Equally apparent but more critical is the terrific concentration in the famous industrial quadrilateral. From Baltimore northeastward to Boston is perhaps the most heavily industrialized belt in the world. The northern margin of the quadrilateral runs from Boston through the middle Connecticut Valley, the Mohawk, Lower Lakes, and on to Minneapolis–St. Paul. Thence the line runs southward to Kansas City, eastward through St. Louis, the Ohio River country, and across to Baltimore.

During the war and postwar years much talk and newspaper space were devoted to new industrial regions developing outside the Northeast. Comparison of 1939 status (white bars) with the position in 1947 (black bars) indicates significant changes in districts in the South and the West Coast. This is especially evident if comparison is made in terms of percentage change (the shorter the white bar in comparison to black, the greater the percentage change). Of more fundamental concern is the measure of industrial intensity in absolute quantities. On such a basis, areas outside the industrial quadrilateral are not impressive. Contrary to some popular opinion, it can be observed that certain of the old districts of the Northeast had percentage changes of sufficient magnitude to prove they still have plenty of vigor. In this matter of percentage changes prewar to postwar, the guess might be hazarded that those areas of greatest growth relative to 1939 may also be the ones to suffer most if our current uncertain deflation turns to depression.

Just one more interpretation of the map—a gruesome flight of imagination. It shows most of the prime targets of the country, all of them aboveground and concentrated in the densely populated East. With long flak-free passage over the tundra and the boreal forest of Canada, coupled with similar flak-free corridors down the great prongs of Lakes Michigan and Huron, what would prevent at least a few enemy bombers from breaking through our fighter interception?

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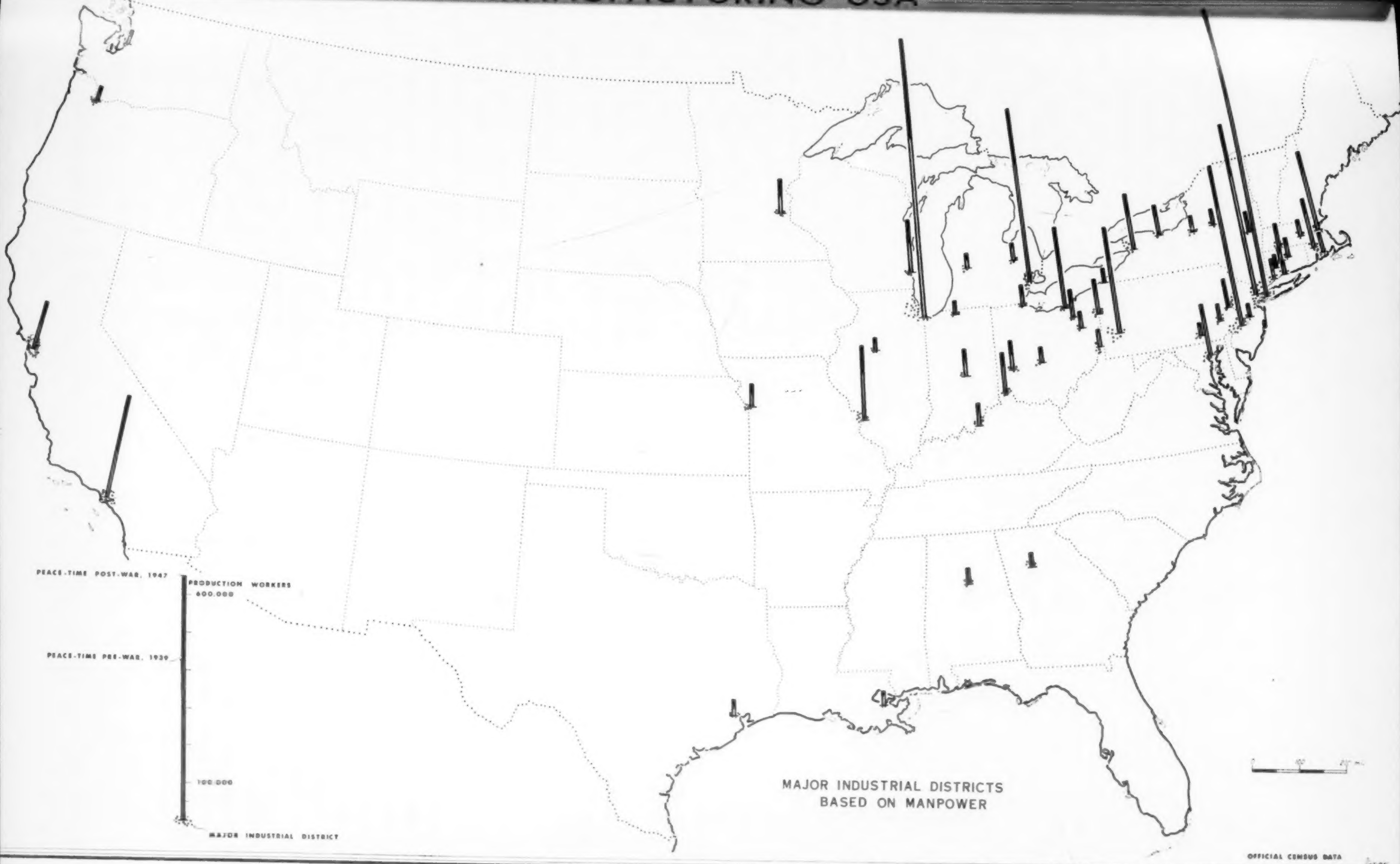

FIG. 1. Industry—its uneven geographical intensity is one of the most critical aspects of our economy.

DESERT RECLAMATION IN SOUTH AUSTRALIA

ANOTHER milestone has recently been passed by Australian soil scientists in their efforts to render substantial tracts of poor land habitable and productive through the elimination of trace element deficiencies in certain of the soils of southern Australia. Reference has already been made by the writer to the existence and correction of cobalt, copper, and manganese deficiencies in Western Australia,[1] and of cobalt, copper, zinc, and molybdenum in South Australia.[2] We now are informed that relatively small amounts of copper sulphate and zinc sulphate, used in conjunction with dressings of superphosphate, are keys to reclaiming more than two million acres of waste land in the Ninety-Mile Desert of southeastern South Australia and in the contiguous Little Desert across the state boundary in Victoria.[3] Most of the land that has been given such treatment, and that until recently has been considered useless, now carries over one sheep to the acre. This carrying capacity is expected to be doubled within a year as the nitrogen content of the soil is built up in response to increased growth of subterranean clover (*Trifolium subterraneum*) in the pasture plant complex. Alternatively, yields of wheat and other cereals are expected to rise from practically zero (without treatment) to thirty or more bushels per acre with this treatment.

This discovery is of considerable import to Australia, a country where good soil and reliable, abundant water supply do not generally occur in combination. It ranks with such earlier discoveries as that a single ounce of molybdenum per acre, mixed and spread with the usual superphosphate top dressing, will increase manyfold the stock-carrying capacity of tens of thousands of acres of land on Kangaroo Island and parts of the adjacent mainland of South Australia; and that an insect mite (*Cactoblastus*) imported from Brazil by Australian entomologists in the 1930s was the solution to reclaiming for grazing purposes more than ten million acres of land in east-central Queensland that was heavily infested with prickly pear and all but useless. In the latter case, the tiny insects achieved what human muscle and bulldozers had previously failed to do—rid otherwise good land of cactus at negligible cost. When the pear was consumed, moreover, the insects vanished.

Credit for finding the means of reclaiming the Ninety-Mile and Little deserts goes to soil scientists of the Council for Scientific and Industrial Research, headed by D. S. Riceman, of the Waite Agricultural Research Institute in Adelaide. Five years of patient, often discouraging, field and laboratory testing of soils and plants in soils went by before the deserts gave up their secret. It is now believed that at least half the 6,850 square miles these deserts occupy can ultimately be reclaimed for agriculture and grazing, and that this reclaimed area will provide settlement opportunities for several thousand farm and pastoral families, or perhaps 12,000–15,000 people directly associated with farm and pastoral lease production and many more in subsidiary or processing industries.

The soils of the region are mostly light, sandy "mallee" soils, with a rather fine texture. In their natural state they support a poor growth of mallee (dwarf Eucalyptus species) heath, bottle brush, broom, honeysuckle, and yacca. Despite this poor vegetal cover, the area receives from 15 inches of rain annually in the north to 20 inches in the south, and an important feature of this rainfall is that it is relatively reliable. The effective growing season is from six to seven and one-half months. Summers are long, hot, and dry, but winters are cool and moist, and fall and spring are warm, with well-scattered rains tapering off toward summer and picking up again in mid- or late fall.

Thousands of acres of land in the region we[re] cleared and planted to wheat in the 1920s under the stimulus of high wheat prices and land settlement boom. Such land quickly became exhausted, however, and crops and pasture grasses failed in spite of heavy applications of superphosphate. Most farms were abandoned, to revert to heath and yacca.

C. S. I. R. soil scientists began experimenting in the Keith District about 150 miles east of Adelaide in 1944. Since that time more than a thousand scattered experimental plots have been established, and samples of their soil carefully studied in the laboratories of the Division of Biochemistry and General Nutrition. It has now been clearly shown, we are told, that 7 pounds of copper sulphate and an equal amount of zinc sulphate, mixed and spread with a bag of superphosphate per acre, will be needed every four years, and the same amount of superphosphate every year. Cost of the zinc and copper treatment is "a few shillings per acre."

The program of building soil fertility in this case begins with sowing of subterranean clover and other pasture plants such as *Phalaris* spp. with a cover crop of cereals, along with the initial applications of zinc, copper, and superphosphate. Two or three years later a second crop of cereals is sown, and these two will usually repay the costs of clear-

ing, cultivation, and seeding; hence, a good-quality, permanent pasture is achieved at low cost. Most pastures developed in this manner to date combine subterranean clover, *Phalaris,* and lucerne as principal ingredients of the sward, but veldt grass, a South African importation already well established in Western Australia, looks promising also. One Keith, S. A., farmer already has 500 acres of this grass for the sale of seed, which is in considerable demand.

One naturally asks, "How is settlement of this 'new' land to be organized and executed?" South Australia's government had not decided this question by February 1949, although there has been a rush of applicants to take up property in the area. The few settlers already on the ground are mostly strung out along the Melbourne-Adelaide railway, and still experience difficulty in trucking farm supplies over the rough, sandy tracks that pass for roads in that region. Good roads and other communications are sorely needed. Most of the land, it is worth noting, is held under perpetual lease at an annual rental of a farthing an acre! Australia's largest life assurance company, Australian Mutual Provident Society, was recently reported to be negotiating for acquisition of more than 100,000 acres for developmental purposes, and hoping to provide essential subdivision, fencing, houses, sheds, pastures, and financial assistance before settlers arrive on their properties.

Obviously the addition of two million acres of productive land to the cultivated area of Australia is not going to relieve the population pressure or food deficiencies of Asiatic countries already facing the specter of Malthus. Equally obvious, it raises the significant question, "How and where will Australia dispose of additional food surpluses taken off these farms and pastoral properties, at prices that will repay costs of development and at the same time provide incentive profits?" At present, perhaps, a hungry Europe, and government subsidies in the form of foreign loans, are sufficient justification for such development. Will this be true next year, or ten years from now? Or will the new settlers be saddled with debts and the Australian government with an additional burden of subsidy?

One hopeful feature would appear to be the cheap acquisition by South Australia of a considerable tract of potentially productive land free from the hazards associated with light and uncertain rainfall. If food surpluses become a state and national burden, it would seem possible and perhaps desirable to relocate some of the rural inhabitants of other, climatically handicapped parts of the state in this area of relative attraction, where cereal crops are likely to be less of a gamble and livestock farming can be added for purposes of diversification. Local needs, no doubt, will be considered by the government before the region is opened willy-nilly to bargain-counter land speculators, farmers who already have adequate holdings in other parts of Australia, or migrants from other countries.

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BOOK REVIEWS

NEW TECHNICS

Rehabilitation of the Handicapped. William H. Soden, Ed. xiii + 399 pp. $5.00. Ronald Press. New York.

THIS volume comes at an opportune time, because of the recent increase in interest in problems related to rehabilitation. Some 23,000,000 persons in this country, it is estimated, are handicapped because of disease, injury, or maladjustment. The book's value lies in its approach to rehabilitation from many avenues—medical, psychological, administrative, and social. It should thus have a wide appeal to those dealing with this large segment of the population.

The book is divided into five parts, entitled, respectively, "General Medical and Surgical Technics;" "Neurological Methods;" "Psychiatric Developments;" "Vocational and Social Rehabilitation;" and "Educational and Psychological Trends." The first two parts treat such topics as early ambulation after operation, chronic arthritis, geriatrics, industrial casualties, physical medicine, plastic surgery, the amputee, the tuberculous, the cerebral palsied, the epileptic, multiple sclerosis, poliomyelitis, and the speech sufferer. Although not wholly devoted to technics per se, the section offers much practical material, describing precise methods for dealing with problems in many of these areas. The nonmedical reader, however, may be at a disadvantage in view of the technical medical terminology employed, since practically all sections are written by medical specialists. But even this might be of value to ancillary personnel desiring further background on the problems with which they may deal. It is no small gratification that the psychological aspects of physical disorder are not lost sight of, and stressed many times over. Indeed, it is rather surprising to find here a statement by a neurologist to the effect that psychosomatic elements may not be discounted in what has been considered a decidedly organic disorder—multiple sclerosis.

The section on psychiatric developments deals with nursing in psychosurgery and in general psychiatric rehabilitation, and describes the comparatively recent movement of Recovery, Inc., a group project for rehabilitating the psychiatric casualty. Particularly refreshing, however, is Dr. Burlingame on "Psychiatric Sense and Nonsense" when he warns against much of a current trend in "wishful thinking and fairy tales" which goes under the guise of scientific psychotherapy. Would that many in this field were as pragmatic as this author!

"Vocational and Social Rehabilitation" is written predominantly by lay administrators of such programs as Community Workshops, Alcoholics Anonymous, Goodwill Industries, the Red Cross, etc., and by those concerned with vocational re-education. The last section discusses such matters as musical therapy, occupational therapy, training of physical educationists, public relations, and psychological aspects. The latter discussion is particularly valuable in suggesting areas for further research in a field as yet little examined by psychologists.

A minor fault of the book is that the index of names is not complete, so that not all names cited appear therein. This reviewer learned much about this vast new field, however, many of the presentations aiding him to separate fact from fiction. The book is recommended to those who are in any way associated with the handicapped, professionally or personally.

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THE INSECT'S HOMER

The Insect World. J. Henri Fabre. xvi + 333 pp. $3.50. Dodd, Mead. New York.

FROM 1879 to 1907 a series of ten books appeared, all with the title *Souvenirs Entomologiques.* Their author, J. Henri Fabre, had reached his eighty-fourth year when the last of them was published—an unknown whose struggles against poverty had had but one end: to have free time to watch living insects. Suddenly, through the activities of Maurice Maeterlinck and others, he was discovered, the importance of his contribution became recognized,

people who had never heard of the "Insect's Homer" before, began reading his books at home and abroad. Scientific societies in London, Brussels, Stockholm, Geneva and St. Petersburg elected him to membership. The government bestowed upon him an annual pension of $400. The President of France journeyed to Sérignan to meet its first citizen. After the long years of poverty, of labor, of niggardly recognition, Fabre, nearing his ninetieth year, saw with his failing eyesight the sunshine of brilliant acclaim.

Alexander Teixeira de Mattos rendered Fabre's prose into English, and it is careful selections from these translations that Edwin Way Teale has drawn together with a sympathetic understanding. Fabre's own story speaks out from the pages. It is factual, with few conclusions drawn. With utmost patience he watched the doings of caterpillars and wasps, beetles and moths, scorpions and spiders, crickets and mantes. The beautiful and the ugly are human points of view; Fabre recognized each in the other as sexton beetles buried a dead bird as food for beetle young, or wasps dug a complex burrow to be stocked with flies on which wasp maggots would feed. These are details of behavior one cannot learn from dead specimens. They include intricacies of conduct and inflexibilities of instinct that tell much of the nervous mechanisms in these fragile little animals. Though Fabre's writing is a distillate of his observations

through weeks and years, his enthusiasm maintains a steady fire of interest for the reader. It is contagious enthusiasm, making one pause before a spider web, peer curiously into a pond, question the lives under a stone in the garden. For, if Fabre could find so much on so barren a two-acre plot, anyone should be able to do better elsewhere with comparable patience and care. Nor were his experiments anything requiring special skill or equipment. They were obvious yet illuminating. Teale's selections include many that any child can try, as a double distillate of delightful descriptions, of thoughtful observations, as applicable to America as to the French countryside. They will do much to sustain the influence on coming students of natural history of a schoolmaster who was selfless in his interest in living things.

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FOREST RESOURCES OF THE WORLD

Green Glory. Richard St. Barbe Baker. 253 pp. Illus. $3.50. A. A. Wyn. New York.

MR. ST. BARBE BAKER, a British forester who has travelled extensively throughout the forests of the world, has given us interesting descriptions of each continent's timber resources. In the tradition of Osborn's *Our Plundered Planet* and Vogt's *Road to Survival*, he sounds numerous warnings against the rate of forest exploitation and depletion now going on throughout the world, and urges rapid and widespread application of sustained-yield forest management.

Actually, *Green Glory* is more a volume of forest aesthetics than a treatise on forest economics. It contains no statistical tables or technical terminology; hence the book may be read with understanding by anyone interested in the conservation of renewable natural resources.

But a word of caution is in order. In his commendable zeal to popularize the forest conservation movement, the author unfortunately indulges in occasional oversimplification. A New Sahara, for example, is a chapter descriptive of the march of logging and lumbering across America and the unsound land utilization practices which produced the Dust Bowl of the 1930s. It is more dramatic than realistic. To say, as he does, that the South was depleted of its "areas of rich pine" by 1910 is to ignore the thousands of sawmills and the dozens of pulp and paper mills now operating in the eleven Southern states. Under good forest management, which is slowly spreading throughout this region, the South can permanently sustain even greater forest industries. Moreover, the chapters devoted to forestry developments in America are dated in that even the latest conditions described are those that obtained at least a decade or two ago.

In summing up, Baker proposes a world afforestation program to be undertaken under the guidance of the United Nations. Although he does not suggest the practice of silviculture by law, he advocates adequate conservation legislation in every country, both for timber production and for the protection of watersheds.

The numerous photographic illustrations have been carefully selected and beautifully reproduced.

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BRIDGE TO THE MOON

The Conquest of Space. Willy Ley. Paintings by Chesley Bonestell. 160 pp. $3.95. Viking Press. New York.

IN THE early days of scientific development books were often profusely illustrated. A scientific treatise was not only informative but also a work of art. The revival of this old custom has been successfully achieved in this book. Mr. Bonestell is not only an architect, astronomer, and artist, but also exhibits the well-developed imagination so necessary for this type of work. His illustrations cover imaginary and accurate views of the earth from a rocket ship and scenes showing how we think the surfaces of the planets and their satellites should look to an observer from earth. The text by Mr. Ley is a very lucid discussion of the solar system and of some of the problems connected with space flight. The eighteen figures used to explain these problems give an idea of the scope of future rocketry and astronomy. Two tables are included that are probably the most comprehensive of their type to be found anywhere. Their titles are self-explanatory: "The Planets of the Solar System" and "The Satellites of the Solar System."

The four chapters in the book are as intriguing in title as a mystery story. Chapter I, Four, Three, Two, One . . . Rocket Away! is a descriptive survey of our attempts in the near future for very high-altitude rockets. The altitude of two hundred fifty miles reached with a two-step rocket can easily be extended. A discussion of the orbital rocket—sometimes called a space station or earth satellite—is especially pertinent in view of the interest shown by our guided-missile program in such a project.

Chapter II, Target for Tonight: Luna! takes up the next logical step in rocketry. A good historical background of telescopic exploration of the moon and a discussion of the theories of the origin of the moon's craters give the reader a taste of what to expect whenever a manned rocket actually lands on the moon. It is pointed out that the meteor-impact hypothesis has the backing of experiments with models in the laboratory.

Chapter III, The Solar Family, brings the reader up to date regarding the physical conditions of the planets of the solar system. The conditions described and expected are as accurate as can be obtained by our present-day methods of observation.

Chapter IV, Vermin of the Skies, refers to asteroids, and their number makes the epithet appropriate. The historical treatment of the asteroids reads like fiction but is really fact.

The combination of descriptive astronomy, rocketry,